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HABIT.[1]

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A habit, from the standpoint of psychology, is a more or less fixed way of thinking, willing, or feeling acquired through previous repetition of a mental experience. A freshman goes up to college with little love for the institution. He soon assumes the conventional attitude of "loyalty to Alma Mater." The experience is repeated time and again through his college life, and this feeling attitude becomes habitual. The habit, strictly speaking, is the similar form as regards feeling which consciousness repeatedly takes; it is the fixed way in which the stream of mind flows when these familiar feeling processes form its content. It is not the familiar feeling consciousness itself; it is simply the proceeding of mind in such a way that the familiar processes are in consciousness.

The physician acquires a volitional habit of taking the pulse and asking patients certain questions. The habit is the familiar way in which his consciousness runs its course during a diagnosis. It is not the familiar consciousness corresponding to the diagnosis. It is the familiar shaping which consciousness takes, as distinguished from the processes which make it up. The conscious habitual content is not unimportant in a study of habit, as we shall see presently; but habit in itself lies outside consciousness. It implies simply an accustomed way of reacting.

Again, the lawyer has a habit of thinking in terms of law. If a problem is presented, he thinks at once of its legal aspects, and his conclusions are reached by the logic of law. In this

[1] From the Psychological Seminary of Cornell University.

habit of thought, as in the habits of volition and of feeling just cited, habit is a particular shaping which consciousness shows. It is not a conscious process or processes. To express in a word that habit means the accustomed way of feeling, willing, or doing; the more or less fixed course of consciousness with repeated experiences; the shaping of the familiar consciousness rather than that consciousness itself; one may say that habit is the mode of mental functioning when repeated processes are in mind. Just as memory is, strictly, the way in which the mind reacts when we are remembering, and not the conscious content remembered, and just as imagination is the peculiar shaping of consciousness when we build air castles and create poetic thought, so habit is the way consciousness runs its course when familiar processes are experienced. We may define habit, therefore, as that mode of mental functioning in which repeated processes are predominant in consciousness. Psychology commonly considers volition, feeling, and intellection, the three typical functions of mind. Every mental experience may be subsumed under one of these three categories. Alongside this tripartite classification, there may be placed another division, habitual and non-habitual functioning. Under the former are included all repeated mental experiences; under the latter, all novel experiences. It may be reiterated that these functions are not themselves conscious processes; they are simply types of consciousness, forms of reacting, which find expression in the arrangement or distribution of the processes of consciousness. Every mental process may be classed therefore as the expression of a function of volition, intellection, or feeling; and, at the same time, of habitual or non-habitual functioning. This view of habit as the mental function, or general type of mentation, under which are to be included all repeated conscious experiences, will serve to give us our bearings as we consider some of the literature on habit.

Literature on Habit.

To review, even in a cursory way, all that has been written on habit, lies outside the limits of this paper. We can only indicate here its place in modern psychology. In preface, it may be noted that two distinct general standpoints are coming to be recognized in psychology. There is on the one hand a psychology of structure which studies consciousness as a structure, abstracting from its relations to the individual and his environment, resolves it into structural elements, sensations and affections, and aims to discover their complexes and interconnections; there is on the other hand, a psychology of function which regards conscious processes as functions of the psychophysical organism, just as digestion is a function, with

objective reference in the environment, and with meaning to the organism. This distinction will be illuminating in our study of habit.

James' discussion of habit seems on the whole the most adequate.[1] Yet it is exclusively a functional treatment of the problem. The basis of habit, he says, is physiological. Consciousness has for its substrate the higher nervous centers which "grow to the modes in which they have been exercised." When a mental process passes through the mind, a characteristic nervous excitation occurs in the brain. The latter in its passage through the nervous tissue leaves a trace which facilitates the repetition of the same nervous process. Hence, the corresponding mental process is likely to reappear. With further repetition, the path in the nervous centers deepens, and its mental correlate is experienced with corresponding greater ease. As to the results of habit, he notes, (1) that with actions, it "simplifies the movements required to achieve a given result, makes them more accurate, and diminishes fatigue;" (2) that, on the side of consciousness, it "diminishes the conscious attention with which our acts are performed." The value of habit to the psychophysical organism is emphasized, in that it makes automatic and subject to routine the greater part of the actions, attitudes, and mental processes of daily life. Three conditions favoring the development of habit are stated: (1) "We must launch ourselves with as strong and decided an initiative as possible." (2) "Never suffer an exception to occur till the new habit is securely rooted in your life." (3) "Seize every opportunity to act on every resolution you make, and on every emotional prompting you may experience in the direction of habits you aspire to gain."

In this systematic treatment of habit, James throws the entire emphasis on the functional side of the problem. Habit is discussed in its meaning to the individual, as it manifests itself in the routine of daily life, in such accustomed movements as walking and dancing, and in the habitual ways of thinking and reacting which give expression to character. All this is mind considered, not as structure, but with reference to the individual and his environment. James does show that habit means diminished attention, and that habituated complex movements run their course as a chain of processes in which the

[1] James: Principles of Psychology, 1890, I, 104-127. The treatment of habit given by James might be criticised from the standpoint of psychological exposition, for its emphasis upon the "ethical implications of habit," a characteristic due doubtless to its original publication as a magazine article for popular reading. This very characteristic, however, makes James' chapter on habit, the more valuable for certain readers, *e. g.*, the teacher.

perception arising from each link acts, without conscious direction, as a cue and stimulus to the next. These last facts might be given a structural interpretation, but James' point of view is functional, his exposition is of the meaning of these facts to the psychophysical organism. There remains unsolved by James' statement, the structural problem: what does habit mean for consciousness; does consciousness show one pattern when functioning habitually and another when functioning non-habitually?

The accounts of habit by Sully and Stout also merit attention. Sully says, all "repeated or recurring processes of thought and action become more perfectly organized, and as a consequence, more rapid and unconscious or automatic. This result is expressed by the term habit."[1] The basis for habit, as with James, is physiological. He says that "the characteristic note of habit is mechanicality;" and that "the volitional process in its complete, fully conscious form is restricted to new, or, at least infrequent actions." Sully gives as criteria of the oncoming of habit: (1) Repetition tends to remove all sense of effort, and to render the movement easy, through muscular and nervous adjustment; (2) there is a consolidation of the processses of association involved.[2] He names four criteria indicative of the degree of co-ordination of a habit and suggests that the strength of habits may be evaluated by them.[3] The criteria are: (1) Lapse of psychical initiation for habit; (2) Specialty or precision of response; (3) Uniformity or unfailingness of response; (4) Difficulty of modifying habitual reactions. These four characteristics manifest themselves in habit, but it would be a difficult task to measure habits by them. Sully contents himself with saying that with these criteria, the highest place in the scale of habitual co-ordinations is that of "the secondarily automatic type of movement, as when one takes out his latch-key at a wrong door. From this downward, there is a series of manifestations of habit with less and less of the characteristics just dwelt upon." This solution of the problem of a classification of habits seems hardly satisfactory, and we shall recur to it later.

Sully gives three main conditions which determine the development of habit:[4] (1) The amount of time and attention given to the movement to make it our own. (2) The frequency with which the particular stimulus has been followed by the particular movement. (3) The unbroken uniformity of past responses. Whether this statement of conditions is adequate,

[1] Sully: *Human Mind*, New York, 1892, I, 56.
[2] *Ibid.*, II, 225-6.
[3] *Ibid.*, II, 228.
[4] *Ibid.*, II, 230.

may be left open here; at any rate, since habit is given a physiological basis, these conditions should be given a physiological explanation. We shall return to this point later. Sully mentions another feature of habit which we can, however, dismiss as falling outside the scope of this paper, namely, the bearing of habit on mental development.[1] Baldwin has made habit one of the two primary laws of the growth of mind.[2] The organism is experiencing repeated processes, and novel processes. Repeated processes are organized in the nervous substrate of mind and come to be functioned without the direction of consciousness. This acquired facility with repeated processes persists as fixed mental capital. Mind can then proceed to accommodate itself consciously to new adjustments, which in their turn are learned and passed over to habit. So the psychophysical organism makes mental progress. This relation of habit to mental development is of course a matter of genetic psychology, and is to be omitted here as our standpoint is that of general psychology.

Stout gives four characteristics of fixed habits:[3] 1. Uniformity. 2. Facility. 3. Propensity: "we are prone to do what we are used to do." The proneness has two manifestations: (a) The more fixed the habit, the slighter the cue necessary and the less liable is the reaction to disturbance. (b) When customary action is interrupted or repressed, the propensity becomes conscious desire. 4. Habitual action is independent of attention.

Stout's statement of the conditions governing the development of habit, notes two factors:[4] (1) "The tendency of any mental process to repeat itself—a tendency which grows stronger, the more frequently the process recurs." (2) The teleological disappearance of conation and attention as the habit is learned, *i. e.*, as the mental processess pass from the attentive to the automatic form. Stout finds a physiological explanation for this second factor in "the tendency of neural systems to a state of virtual stability." A mental reaction involves a disturbance of nervous equilibrium, and conative processes, as in the first steps of learning a habit, represent the regaining of equilibrium. The final learning of an habitual action, so that it is functioned mechanically, means the acquiring of a new center of nervous equilibrium appropriate to the disturbance made by the repeated action. The action now no longer disturbs nervous equilibrium sufficiently to give a basis

[1] *Ibid.*, I, 201.
[2] J. M. Baldwin: *Mental Development in the Child and Race*, 1895, p. 214.
[3] G. F. Stout: *Analytic Psychology*, London, 1896. I, 258.
[4] *Ibid.*, pp. 263 ff.

for conative processes. We shall recur later to the problem of a physiological explanation of habit.

The psychologists so far quoted, James, Sully, and Stout, approach mind from the functional standpoint. Let us hear a structural psychologist as well. Kuelpe, in his "Outlines," gives no systematic treatment of habit. His general problem is:[1] (1) to isolate and classify the mental structural elements, the various sensations and affections; (2) to classify complex processes, as made up of interconnected elements; (3) to investigate the state of consciousness as presenting general differences independent of the contents of experience. With such a programme, he introduces habit solely as one of several factors conditioning sensitivity and sensible discrimination, and hence, to be taken into account in their introspection and measurement. Habituation he defines as a "predisposition of consciousness," or "a tendency taking shape in a series of similar observations to experience and describe perceptions of similar character. . . . . In such a series we are apt to find a certain direction and degree of the attention growing habitual and a particular category of judgment becoming preferred."[2] An illustration of what habit means for his system, is given in his inclusion of habituation among the general conditions of tonal fusion;[3] he states that it "lends an added distinctness to the total impression, or the individual constituents of a connection, according as one or the other has been the object of repeated judgment or perception." In another reference to habituation, it is given as one of the determining conditions of feeling, since "under its influence both pleasantness and unpleasantness approach indifference."[4] Kuelpe's only concern with habit is its influence on the structural analysis of mind into elements and their complexes. Mental processes which are repeated a few times come to show slight differences from the form in which they are first experienced. For exact psychophysical determinations, it is necessary to evaluate, or, at least, take into account, such differences. That is all habit means to Kuelpe's system. Were his exposition of the state of consciousness more complete, one might expect further structural reference to habit in an account of differences between the habitual and non-habitual states of consciousness. Neither Kuelpe nor others give such a statement, however. We shall return to it immediately as an important problem regarding habit.

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[1] O. Kuelpe: *Outlines of Psychology*, Trans. by E. B. Titchener, New York, 1895, p. 19.
[2] *Ibid.*, pp. 39, 41.
[3] *Ibid.*, p. 303.
[4] *Ibid.*, p. 261.

There are other writers who might profitably be examined. Enough have been cited, however, to furnish the broad outlines of the subject. Summarizing, it is evident that the functional psychologists have given the subject most attention. The quotations cited have set forth the basis of habit, the conditions of its development, its characteristics, and its meaning for the individual. Kuelpe, a typical structural psychologist, considers habit only as helping or hindering in the analysis of mind into structural elements and complexes. In this examination of the literature of habit, various problems have presented themselves. Of these, we will consider the following three: I. The differences between the habitual and the non-habitual states of consciousness: (A) as regards pattern or form of arrangement of parts; (B) as regards specific processes present in the one and not in the other. II. A classification of habits. III. The development of habits.

The State of Consciousness with Habitual Functioning.

Habit, as we have seen, is a particular mode of mental functioning, antithetical to non-habit. Every mental process has either been in consciousness before, or is entirely novel. Processes of the first sort are included under habitual functioning; of the second, under non-habitual functioning. The present enquiry asks what are the differences between the states of consciousness with these two kinds of functioning. The enquiry breaks into two parts.

A. *The Difference in Pattern or in Arrangement of Parts.*

(1) The first distinction to be included under this head is a quantitative one: the conscious processes of habitual functioning are few and meager as compared with those of non-habitual functioning. Non-habitual processes bulk large in consciousness; habitual processes bulk small. Compare for example the mental processes with the familiar act of writing one's signature and those with the non-habitual act of writing words backward. Examining the former consciousness, one finds the conscious impulse which started the signature writing and the mass of tactual, movement and visual perceptions set up by its movements. There is no series of conscious impulses initiating the part movements of the writing, or any conscious direction of them. One may assume, with James, that each part movement is set off, without volition, by the vague, unattended-to perception arising from the preceding part movement. Let us examine these two states of consciousness more in detail with a view to quantitative differences.

One may examine the habitual consciousness with signature

writing in three ways. (a) By attending to the writing in a general way, it becomes the most prominent part of consciousness. The processes prominent, however, are not the impulses directing the part movements, or the perceptions arising from the part movements. That is, the processes of the habitual functioning itself do not stand out in consciousness. The prominent processes are the mass of resulting perceptions, more or less differentiated into parts, or some suggested process, as a thought regarding the purpose of the writing, the peculiar shape of the letters, or a memory of writing the signature in starting a bank account. The processes which actually direct the part movements are meager and loom small in consciousness. (b) If attention be concentrated closely on the conscious direction of the part movements in the habitual writing, and all other processes be inhibited, in an attempt to make these habitual processes large in consciousness, one finds the habitual functioning changed into non-habitual. There are at once checkings to the smooth, mechanical course of the habit, and the writing is marked with effort. It is impossible to bring the mental processes of habit into prominence in consciousness, and maintain the habitual form of functioning. (c) If attention is directed in neither of these ways, the sparse processes which direct the habitual part-movements drop out, and other processes quite unrelated to the signature fill the field of consciousness. So, while writing, I spoke to a friend who came into the room, listened to the wind outside, and experienced other extraneous mental processes. The facts, that on the one hand, the familiar writing goes on uninterrupted by related and unrelated simultaneous processes, and, on the other, that it is impossible to make the direction of habitual movements prominent in consciousness without destroying the habitual form of functioning, indicate the small bulk of the conscious processes in habitual functioning.

In non-habitual functioning, on the contrary, as in writing words backward, the mental processes accompanying the act stand out prominently. There is a tangle of processes including the selecting of letters to be written, the rejecting of others, and visual and auditory images of the words whose letter-order is being reversed. The enumeration of these processes indicates their bulk. They fill consciousness completely, excluding other processes unless these be habitual. So, for example, the tangle of non-habitual processes involved in choosing letters in an unusual order, is accompanied by the slightly conscious habitual processes of writing the letters when chosen. Additional non-habitual functioning, however, is excluded. While writing words backward, some one spoke to me and the unfamiliar movements stopped until the interruption was past.

With signature writing, however, the habitual processes ran their course undisturbed by the presence of other processes, as of speech, audition, and memory. This fact of itself, indicates the relative meagerness of the mental processes with habitual functioning, and the fullness of processes with non-habitual functioning.

It is a fair question how "full" the non-habitual process, and how "meager" the habitual process may become. The former may occupy practically the entire field of consciousness as in the scientist's absorbed thought on some new problem, or in an instant of complete concentration with the beginner in dancing. On the other hand, the habitual processes may move temporarily outside the field of consciousness, as when while walking one becomes entirely "wrapped up" in some problem, and yet continues to walk, though unconscious of the movements. In such a case, the perceptions of part movements which serve each as a cue to the succeeding part movement, become so vague that they may be regarded as subliminal excitations. This again indicates the meagerness of the conscious processes in habitual functioning.

Concluding this point, it seems established that mental processes with habitual functioning are few and meager compared with those of non-habitual functioning. With the former, other processes in consciousness are possible and usual; with the latter, other processes, unless habitual, are excluded.

One may think of the difference between the habitual and non-habitual consciousness in terms of a cross-section of mind. Then the habitual consciousness in cross-section shows a small low wave representing the meager habitual processes, and in addition a larger wave representing the incidental processes, habitual or non-habitual, which may also be present. Were one to illustrate this graphically, one would represent the small wave in heavy lines—to indicate the determining importance of the habitual processes—and the large wave in light or dotted lines, to indicate the incidental presence of other processes. The non-habitual consciousness in cross-section shows a large, high wave representing the bulky processes of non-habitual functioning. Graphically, the large wave would be represented in heavy lines, to indicate its importance in the consciousness; the cross-section should also show, in lighter lines, a low wave to indicate habitual processes, small in bulk and in importance, which are also present, usually, in the non-habitual state.

(2) A second difference in the pattern of the habitual and non-habitual states of consciousness, arises from the fact that non-habitual functioning is consciously attended to and habitual functioning is not. This introduces into the former the

distinctions of the attentive state, in which processes attended to stand out clearly and distinctly and those attended-from are less clear and distinct. So we have in the non-habitual consciousness a pattern with two degrees of clearness: the processes of non-habitual functioning are relatively clear and distinct; and other parts of consciousness, the inhibited interfering processes and accompanying habitual processes, are vague and indistinct. The simple habitual consciousness, shows but one degree of clearness; the habitual processes are of the less distinct shading. It should be said, however, that the habitual consciousness is ordinarily not so simple in experience; but includes, in addition to vague habitual processes, others which have the character of non-habitual processes and which stand out with clearness. These differences will come out more distinctly in illustrations. In writing words backward, the non-habitual processes of selecting the letters to be written, not only bulked large in consciousness,—they were clear and distinct. Along with them ran indistinct processes; a vague consciousness of my surroundings, inhibited auditory perceptions, and dim perceptions from the familiar movements of writing. The unfamiliar processes which were attended to had one degree of clearness, they were distinct; the other processes attended-from had another degree, they were dim and obscure. With the familiar signature writing, on the other hand, the processes of the functioning showed but one degree, that of vagueness. If other processes, as those involved in perception, or thought, incidentally came into consciousness during the habitual functioning, they could be attended to, and, as a result, rise into the higher degree of clearness. The habitual processes of themselves, however, remained in the dim, somber background of consciousness.

This distinction in distribution of clearness in the habitual and non-habitual states comes out more plainly in the contrast between the mental side of an occupation like typesetting which involves an habituated routine, both mental and muscular, and one like reporting which deals with a succession of novel situations. Consciousness in the former case is of an almost exclusively habitual type; in the latter, non-habitual processes are prominent. The consciousness of the typesetter who has been doing straight newspaper composition for years, shows in general, we may believe, but one degree of clearness, the dim gray of habitual processes. The sight of the phrase in his copy and of the case before him, the feel of the stick in his hand and the line growing under his thumb, the pressure of the types as he picks them up—these form the mental side of his work and are represented in consciousness by processes of which he is normally scarcely conscious. They give his con-

sciousness a prevailing shading of the less degree of clearness. Occasionally, extraneous ideas, thoughts about the content of the copy, come into mind, or perhaps there is a moment's attentive work in adjusting the spacing of a line, and then, for the moment, processes of the brighter shading stand out against the dim gray of the habituated processes. Take his consciousness the day through, however, and it is essentially a dull monochrome, the characteristic of habitual functioning.

The consciousness accompanying the reporter's work is quite different. It is essentially non-habitual. He must be on the watch for every situation which promises news, and when such a situation is found, he must develop rapidly its every bearing and detail. This non-habitual mental functioning must be done under keen attention. Hence the mental processes involved are at the higher grade of clearness. Other parts of his work, however, the familiar questions used in interviewing, the developing of typical news-situations, are soon matters of routine, and the corresponding mental processes take on the obscure gray of habituation. So his ordinary consciousness shows the two degrees of clearness which mark non-habitual functioning, the clearness of non-habitual processes, and the obscurity of accompanying habitual processes. The typesetter's consciousness, on the other hand, is an habitual consciousness of almost a pure type: its processes show, in general, only the obscure shading of habituation.

(3) A third structural distinction between the habitual and non-habitual consciousness is in regard to the interconnection of part processes. We have noted that in complex habitual movements, like walking, it is enough to start the movements voluntarily. The chain of movements then runs out of itself. I make up my mind to go to the post office and start out. This much involves attention and is a voluntary act. I walk down the street, turn the familiar corners, and, without a thought of my movements, keep my mind busy with other matters till I find myself at my destination. The inception and conclusion of the long series of movements were conscious. But I gave no voluntary direction to the intermediate movements. So, as I write now, I form letters in a particular way, and join certain letters in a definite order to express a word which is in my mind; the word is conscious but I dash off its written symbol, involving a series of delicate, complicated muscular adjustments, and each part movement comes into its proper place smoothly and rapidly without conscious direction. The antithetical characteristic of non-habitual functioning is noticeable in learning any new adjustment, *e. g.*, learning to write in childhood. How slow and painful the child finds it! The up and down strokes and curves are drawn only under dis-

tinctly conscious direction. The pen goes wrong and must be checked by conscious inhibition; it goes right only as proper movements are voluntarily initiated and directed. At every step, right and wrong paths of movement lie open, and the child must choose consciously between them. The adult has a similar experience in learning golf, bicycling, or any set of muscular adjustments. He consciously inhibits wrong movements, and initiates right ones. In short, non-habitual processes are at loose ends, and attention must seek out the proper part movements and connect them up to secure the result desired. The penman and skillful golf player, as we have seen, have organized the part movements, so that once initiated the chain of movements runs through its course, without the hesitations and conscious choosing of paths experienced by the beginner. This characteristic of habitual functioning is to be explained in terms of association; the acquiring of a habit means the acquiring of a fixed series of associations. Each part movement has been welded to the preceding part through repeated co-existence in experience, so that the perception arising from the performance of the earlier acts as a cue to the following part. Each term calls out its succeeding term in the fixed series, and no other processes. By repetition, the complicated movement has become a series of associated links; the performance of the first evokes the second without volitional assistance, the second evokes the third, etc. There is conscious initiation of such a movement and a consciousness of its close, but its performance may become quite unconscious.

Such phrases as the "ease," "rapidity" and "smoothness" of habitual functioning, and its "automatic," "mechanical" and "instinctive" character, have their warrant structurally in the fact that habitual functioning is in terms of a series of organized associations, each term of which calls out its proper successor immediately and unconsciously.

Non-habitual functioning, as one's first effort at skating or piano playing, presents opposite characteristics; it is slow, difficult, uneven. Looked at from the standpoint of association, there is no fixed linking of step to step; each succeeding part movement must be selected consciously. The performance of one part of the movement suggests not a single associated step to follow, as in habit, but many associated steps from which the one most suitable to the end in view must be chosen. The attention, fixing on this one, brings it out clearly in consciousness while other processes are inhibited. The functioning follows the path selected. The same procedure is repeated for the next term—a mass of associated steps is suggested, some more prominently than others. Attention works over the mass, selects the most suitable one, which is followed out, and the

next link in the chain is forged. The association of steps thus joined persists and shows itself in greater facility when the series is repeated. Repetition strengthens the linking of part to part, and in time the series functions mechanically, each part suggesting only the proper succeeding part, and this without volitional direction. The non-habit has become a habit.

The relation of association to habit and non-habit throws light on the apparently anomalous fact, that it is impossible to attend to the parts of an habitual complex and consciously direct them without destroying the habitual functioning, making it non-habitual. Attention to any mental process, according to Kuelpe[1] means physiologically a "preparedness" for its neural correlate. The preparedness consists of two factors: an increased excitability of the cortical path or cells corresponding to the idea, and an opening up of the neural paths of its associated ideas. The neural processes underlying other non-associated ideas are inhibited. As a result of this preparedness, the idea attended-to comes into consciousness more clear and distinct than other ideas, and is rich in associations. Apply this to habitual functioning. Such functioning, as we have seen, is mechanical and rapid, each term unconsciously calls out its habitually succeeding process, and no other processes. Let attention now be focused on the direction of a part movement; the cortex "prepares" for its neural process. The corresponding cortical center is increased in excitability and its associated paths of functioning are opened. With habit, but one such path was opened, the one standing next in the organized series; now, many offer themselves for the successive step in functioning. The situation is exactly as in non-habitual functioning. There are many possible ways of functioning opened and a decision must be made among them. The decision is made as follows: the most "prepared" path, which is the one having among its associated ideas the end being sought, *i. e.*, the one which best leads toward that end, is thereupon attended to and the functioning follows along it. Attention to the part process opened up many possible paths; and, once opened up, a selection from them by means of attention was necessary. This arousal of possible paths and choice from them is repeated for each term as long as attention is focused on the part processes of the habit. But this is non-habitual functioning—slow, conscious, uneven. Habit becomes non-habit under attention, because attention to a part process arouses the processes associated with it, and functioning through such a tangle of processes is necessarily non-habitual.

Another fact to be considered in connection with association

[1] *Monist*, October, 1902.

is the initiation required to set off an habitual reaction. Sometimes volition is present. I decide to go walking, or to go to bed, or to eat lunch, and the habitual complex starts and thereafter runs, at least in its mechanical parts, without conscious direction. In completely mechanized habits, however, no active volitional initiation is necessary. A perception or idea quite unattended to may evoke the habitual response if it is not volitionally inhibited. I rarely go to my sleeping room save at night, and then always throw on the electric light as I enter. On two or three occasions when entering during the day and busy in thought with something else, I have unconsciously turned on the light. The perception aroused by entering the room had been habitually associated with the movement, under one set of conditions, darkness; and the reaction followed even under irregular conditions. Ordinarily it would have been inhibited, but volitional processes were focused elsewhere. There is always some stimulus to an habitual reaction, and as this illustration shows, it need not be volitional. Some perception or idea which has been in the past associated with the habitual function is sufficient, even if unattended to, and scarcely conscious. Volition, in such cases, is concerned only negatively: it must not inhibit the reaction.

It may be said, without offering details, that to the fact that habit is an organized series of associations is due some of its characteristics which Sully and Stout note: its "uniformity," "unfailingness," "specialty or precision in response," and its "propensity" in so far as this last term indicates the readiness of a habit to respond to a slight cue. We shall say more of propensity presently.

Summarizing this account of the differences in pattern in the habitual and non-habitual states of consciousness, we may say that in the habitual state the processes are (1) quantitatively meager, (2) uniformly indistinct, and (3) are interconnected as a series of associations so that each term unconsciously evokes the succeeding term; in the non-habitual state, the processes (1) bulk large in consciousness, (2) are of two grades of distinctness according as they are attended-to or attended-from, and (3) are joined together in the functional series only as a result of conscious choosing and rejecting.

B. *Distinctive Processes of the Habitual and Non-Habitual Consciousness.*

The attentive state, over and above its characteristic pattern of distinct processes at the focus and indistinct processes at the edge of consciousness, has a characteristic process, effort, which distinguishes the attentive from the non-attentive state of consciousness. Our second and present enquiry is regarding

processes which in a similar way may be peculiar to the habitual and non-habitual states.

(1) The distinctive characteristic of the habitual consciousness is its feeling-tone or mood. When a process reappears in consciousness it is accompanied with a mood of familiarity, or "at-homeness." With further repetition, as the process becomes thoroughly habituated and mechanical, the mood shades off into vague and scarcely-conscious indifference or "of-courseness." For example, I am introduced to a stranger and next day meet him on the street. His name, a repeated mental process and hence slightly habitual, flashes into mind, and the mental processes include a mood of familiarity. Let me meet him daily and the mood of familiarity accompanying his name in consciousness speedily diminishes in intensity. I soon speak his name mechanically, and as "a matter of course" and the accompanying consciousness is quite indifferent in feeling-tone. In a similar way, as one repeats a trip through a strange part of a city, the processes called out by the sight of business houses seen before and by following the turns previously made, are marked by a feeling-tone of familiarity. Further repetition of the walk reduces the intensity of the mood and it passes over into indifference. So one comes to use the rules of arithmetic, pass to and fro in accustomed surroundings, recall the familiar names of friends and go through entirely habituated bodily adjustments, and in all such habitual functioning, the mental processes are accompanied by no distinct feeling-tone. They are quite indifferent. Yet they may be regarded as contributing positively to the vague feeling-mass of "naturalness" or "of-courseness" which forms the dim background of consciousness. A hint of what the indifferent of-courseness of habit means to mind may be had when an habitual function is interrupted, and a feeling of unpleasantness due to the interruption succeeds the of-courseness. If a familiar object is removed from one's room, if one's daily routine is interrupted, or if a name which one uses familiarly suddenly goes from mind, each such interruption of habitual functioning is accompanied by an unpleasant feeling. At the moment this succeeds the mood of "of-courseness," one is conscious that the latter contributes something to the vague feeling of naturalness in the background of consciousness. Habitual functioning is therefore characterized consciously by the feeling of familiarity; and in thoroughly organized habits, by the indifferent mood, termed "of-courseness" or "naturalness." Interruptions of habitual functioning are unpleasant in feeling-tone. This gives us the distinctive process in the habitual state of consciousness.

This affective characteristic explains in part the "impulsive-

ness'' or ''propensity'' of habit, which we may consider for a moment. Reid[1] says that habits often involve not only facility in action but ''inclination or impulse toward action,'' and Stout, as we have noticed, includes ''propensity'' as a characteristic of habit. This characteristic seems explicable by two factors. (a) The first is that of association, already mentioned. Each term of the habitual series calls out its succeeding term without volition, and the series may be initiated without volition by the presence of the perception or idea which has been associated with it as its cue. This fact accounts in part for ''propensity'' toward habitual functioning. Such functioning is in the line of the least resistance, both as to initiation and progress. A situation is presented in which one may speak truly or falsely. If in one's past experience truth-speaking has been the next associative term in similar situations, it is easier now to speak truly than falsely. Truth-speaking is unconsciously suggested as the next term in the functioning, and this term follows without volitional direction. In a similar way, if the hour of five has been associated daily with taking a walk, the arrival of that hour to-day suggests the walk. It is easier to follow the habituated association than to form a new one. We may say, then, that the propensity toward habit is explained in part by the slight initiation it requires and by its non-volitional course, both of which are referable to the fixity of association. (b) The other factor is the affective processes involved in habit. Habitual functioning is accompanied by the mood of familiarity, or, if the habit is mechanized, by the vague mood of ''naturalness'' or ''of-courseness.'' If habitual functioning is interrupted, there is a feeling-tone of unpleasantness. These affective processes both favor habitual functioning, as against non-habitual. Telling the truth contributes to the mood of naturalness. If one hesitates to speak the truth, habitual functioning is interrupted and the unpleasant feeling resulting impels to truth-speaking. False-speaking similarly breaks in on habitual functioning and is therefore unpleasant. The affective processes favor the habitual functioning, the truth-telling. Just so, with the habitual walk: following the regular sequence adds to the vague general feeling of naturalness and of-courseness; breaking the sequence, is unpleasant. Hence there is inclination to follow the habitual order. The ''impulsiveness'' or ''propensity'' of habit is to be explained, then, by the ease of functioning in the associated order, and by the influence of the affective processes concerned.

Summarizing this section, we may say that the distinctive process in the habitual consciousness is its mood of familiarity

[1] Thomas Reid: *Essays on Active Powers*, Essay 3, Chap. 3.

with partly habituated functioning, and of vague indifferent naturalness with entirely habituated functioning. This affective characteristic of habit, together with its organization as an associated series, explains its impulsiveness.

(2) The distinctive process in the non-habitual state of consciousness is the experience of directed effort. This is plainly present in learning a new muscular adjustment, as swimming, typewriting, or the playing of musical instruments. The innervation of the proper muscles, to the proper amount, and in the proper sequence, and the inhibition of false movements, are accompanied with a consciousness of effort. The effort, further, is directed; namely, toward the organization of a series of movements which will give the desired adjustment. The end is conscious and the choosing and rejecting of movements is directed toward accomplishing it. Moreover, the feeling of effort is localized in the muscles, or the tendons of the muscles, which are being adjusted—in the finger muscles in learning the piano, in the leg and arm muscles in learning to swim. There is present, too, the general set or brace of the whole body which is always a factor in effort.

The experience of effort may be traced also in non-habitual functioning of the intellectual type. If I go into a shoe store to inspect a pair of shoes, a series of habitual processes runs through mind corresponding to my questions: "Are these shoes well made? What kind of leather? What sort of a sole? Is this in the spring style? The price? etc." The questions seem to suggest themselves, without any consciousness of effort, when I am in the familiar position of shoe purchaser. I attempted, however, to think myself into the unfamiliar position of the shoe dealer and conceive what judgments he might pass upon the shoes. I could do it only with effort. The end in view was conscious and the necessary steps toward it were made volitionally. The processes included a determination of the points of interest in the shoes for the dealer, and the judgments he might make upon them. I conceived him as thinking: "The cost of these shoes to me? Do they seem to fit and please the customer? My profits if he takes them? The grade of leather in them? Their manufacturer?" I arrived at these unfamiliar mental results only with consciously directed effort. The feeling of effort was localized physiologically about the eyes, as the shoes were examined visually, and the thinking was largely done in visual terms.

Feeling experiences, too, so far as volitional processes are concerned in them, show the same characteristic of the effort factor in non-habitual functioning. The effort, be it noted, attaches itself not to the affective side of the experience but to

the perceptions or ideas present, *i. e.*, just as it is impossible to attend to the affective side of an experience, so it is impossible to make it the basis of effort processes. One has an example of non-habitual functioning in feeling whenever an æsthetic or other judgment of sentiment is made. For example, I see a famous picture for the first time and study it carefully, passing judgment upon it point by point. The mass of processes is affectively toned and the experience is conative. The next time I examine the picture, as far as judgments once made under effort are repeated, they are repeated with diminished effort or without effort. As the picture becomes familiar, the judgments once made are accepted and used without actual conative acts of judgment. In all feeling experiences, the effort which attaches to the perceptions and ideas behind the feeling, is present only in non-habitual functioning. As in other cases, effort is directed toward an end—in the illustration cited, the forming of æsthetic judgments. This applies to one's feeling attitude toward countless things in art, in ethics, in religion, and in intellectual or logical thinking. For every such attitude intelligently taken, and not accepted at second hand, there was an original process of non-habitual conative judgment, affectively toned. As the results of the judgment came to be habitually used, the conative processes dropped out. The feeling attitude is thereafter taken without effort. There are many feeling experiences, the emotions of joy, sorrow, fear, etc., and the feelings proper, warmth, thirst, weariness, etc., in which no such processes of judgment are involved. The attention is passive, and the effort experience as in all passive attention is weak. It would be difficult to differentiate habitual and non-habitual feelings of this sort, by the effort factor. There is another criterion, however, which may be applied to such feelings and indeed to all feeling experiences: non-habitual feelings are relatively intense; with repetition, both pleasantness and unpleasantness approach indifference, whether present in a feeling proper, an emotion, or a judgment of sentiment. This general statement was implicit in our treatment of the feeling-tone of habitual functioning which diminishes in intensity with repetition, passing from the mood of familiarity to that of indifference. Other examples might be cited of this general characteristic of feeling experiences: a new suit of clothes is worn with pleasurable feelings for a few days, but in a couple of weeks it is quite a matter of course and calls out no affective reaction. Unpleasant feelings likewise decrease to indifference. On one occasion I had to work for some time under unpleasant circumstances which irritated me not a little at first. In a week or so, I was disregarding them; they had become indifferent.

Summarizing this discussion, it may be said that the directed effort experience is the distinctive process in non-habitual functioning. With feelings, further, all non-habitual processes are relatively more intense: as they become habitual, they pass to indifference.

Placing together the results of this section, we note that the non-habitual consciousness is distinguished, as just stated, by its effort experience and the relative great intensity of any feeling processes present; the habitual consciousness, by its mood of familiarity for repeated and partly habituated experiences, and of indifferent of-courseness for thoroughly habituated experiences. For the sake of summary, we may recall the differences in pattern noted in the preceding section: the processes of the habitual consciousness bulk small, are uniformly indistinct and are fused together in an associated series so that each term unconsciously calls out its proper succeeding term and only that term. The processes of the non-habitual consciousness bulk large, are of two degrees of clearness, distinct and indistinct, and are consciously joined together as terms in the functioning series by attentive selection and rejection.

A Classification of Habits.

As we stated at the outset, habit is a mode of mental functioning in which repeated processes are in mind. A habit is one such mode of functioning; and there are as many habits as repeated processes. How can we classify them? A preliminary statement of the concept of nervous tendency in its connection with habit will assist us. A nervous tendency is a particular modification of the nervous system favoring a definite sort of functioning. Two men receive the same objective stimulus, from a green field: one thinks, "how fine for a golf course," the other, "what a good pasture it would make." The two nervous systems reacted differently because of their individual peculiarities, the nervous tendencies present in each. Habit, as we shall explain in a later section, is at bottom, a physiological phenomenon. The acquiring of a habit means the development of a persisting nervous modification, a "tendency," which expresses itself in the various conscious manifestations of habit. It will be convenient to speak of an "habitual tendency," meaning the particular neural modifications underlying an habitual mode of functioning. By examining the manifestations of habitual tendencies, one comes to a first basis of classification.

A. *Specific and General Habits.*

Every mental experience possesses in some degree the possi-

bility of reappearance in consciousness. I can recall the mental experiences of the instant just passed, but for most such experiences, the possibility of recall is brief. I remember the dishes at dinner to-night, but I have no idea what was served yesterday. I recall a deep snow in December, but other weather conditions during that month are gone beyond recall. I had to derive a mathematical formula a few days ago which I had not looked at in some years; after working at it a bit, I caught a cue and the derivation almost ran through of itself. In each of these cases where a mental experience is revived, one assumes that the original experience left a trace in the nervous substrate of consciousness making possible the recall of the mental processes. Similarly in learning a muscular adjustment, as tying a new form of neck-scarf, the first performance may be carried out only with difficulty; but the mental processes leave an effect in their substrate, a tendency, which makes a repetition of the same movements easier. Behind the habitual performance of a daily routine, one assumes a set of nervous modifications, developed by earlier performance of the duties, which provide for their machine-like repetition day by day. When I have once formed an opinion on the Sistine Madonna, this opinion comes into mind at once if Raphael's painting is being discussed. I do not wait to form a new opinion; the one which has been in consciousness before, reappears. In each of these cases, a particular set of mental processes is experienced, and an habitual tendency brought about which facilitates the re-experience of the same specific processes. Such a mode of functioning may be termed a specific habit, or, since mental contents are repeated, a habit of content.

Habitual tendencies, aside from providing for the re-experiencing of specific processes, manifest themselves by shaping the course of other processes in consciousness. One day in January the weather bureau reported a fall of 34° in temperature and a wind of 30 miles an hour. The facts were unusual and have come into mind many times since. More than that, for two or three days after the cold snap I was weather-minded. I scanned the papers to watch the progress of the storm elsewhere; I found myself discussing the weather, an unusual topic for me; and I inquired from a friend just back from New York, as to the weather there on the day of our blizzard. The habitual tendency persisting from the original experience manifested itself first in the re-experiencing of the specific processes; and secondly, in a more general way, by giving a certain form to consciousness in accord with the earlier experience. The habitual tendency which underlies truth-telling is of this more general character: it does not provide for the re-experience of specific processes, but determines a general form of func-

tioning which shapes whatever processes may be in mind so that their expression is truthful. Similarly, with the habits of punning and answering letters the day they are received, mentioned by Stout. In each, the habit determines the form of consciousness; in one case punning, in the other answering the letter at once. It does not determine the specific pun or the contents of the answer made to the letter. It is evident that we have here a second form of habitual functioning which may be called a general habit, or since it determines the form of consciousness, a habit of form.

Both sorts of habit find illustration in the "professional mind." On the one hand, the mental processes corresponding to the facts of the profession, *e. g.* law, are at the instant service of consciousness, so strong are the habitual tendencies favoring their re-experience. These are specific habits. On the other hand, the continued experiencing of the mass of related processes, which form the mental side of a profession like law, develops general mental habits in accord with these experiences. The lawyer's thought shows the impress of these habits; it follows legal logic. The memory of the professional man is best for the facts of his profession; he easily masters new situations within his field while he may be incompetent outside it; his interests are those of his profession, so that, if a lawyer, he enjoys Blackstone, while a scientific monograph would bore him. These are evidently manifestations of general habits which give varying form to consciousness in different individuals. Their influence is clearly illustrated in the different reaction which various minds make to the same situation. Let three men, an artist, a farmer, and a railroad man look out over the hills about Ithaca. The first will probably be enraptured with the beauty of the scene, the second will wonder about the quality of the soil on the hill tops, and the third will remark that there must be heavy grades on the railroads entering the city. The artist has in the past experienced mental processes pertaining to the beautiful, and so has acquired a general habit of thinking in terms of the æsthetic. His present mental experiences are shaped by this general habit. Similarly, with the farmer and the railroad man. General habits are of the most far reaching significance as accounting in part at least, for differences in mental constitution, type and temperament. In part, the tendencies that determine these manifestations are doubtless hereditary; very many of them, however, are habitual tendencies built up by past mental functioning, which persist and determine the course of our mental life in the present.

From this point of view habits may be regarded as of two kinds, specific and general. The former function in mem-

ory, imagination, thought, bodily movement, etc., in which specific processes experienced in the past, reappear in consciousness. The latter function in habits of thought, attitude and volition in which novel processes of the present are shaped in accord with the experiences of the past. The bearing of the following statement from James on this distinction, is clear: "It is not simply particular lines of discharge, but general forms of discharge that are grooved out by habit in the brain." There is possible, also, another classification of habits.

B. *Levels of Habit.*

Sully, as we noted in examining his treatment of habit, attempts to measure the "degree of habitual co-ordination," or the strength of habits, by using various characteristics of habit as criteria. But he only succeeds so far as to say that the highest place in the scale is that of "the secondarily automatic type of movement," and that "from this downward, there is a series of manifestations of habit with less and less of these characteristics." We have seen that habitual tendencies manifest themselves in two ways, providing for the re-experiencing of specific mental processes and exerting a shaping influence on consciousness. We might find in the persistence of the former characteristic and in the degree of the latter, the basis for a quantitative classification of habit. Such a measure would be difficult to apply directly. Titchener, however, has suggested a division into "levels of habit"[1] in which the lines of demarcation are primarily in accord with the sources of the habituations. By combining his "levels" with a rough application of the measure just suggested, we arrive at a fairly satisfactory classification of habits.

Habits whose basis is simply recency of occurrence are least persistent, and exert the least influence on consciousness. They form the lowest level of habit. The miscellaneous everyday experiences of only ordinary interest belong here. We remember them for a longer or shorter period varying with the depth of the impression they make upon us. They may also influence one's other conscious life for some little time, as did the unusual weather conditions. There is very little of last year's daily experiences of this sort, however, which is remembered now or which influences consciousness in a way one can be aware of. In other words, the tendencies resulting from this miscellaneous, unrelated daily functioning do not persist for any length of time. Habits of this level may be called temporary habits. Every one has the experience, however, of some scene of childhood or later life which does persist, both being

[1] E. B. Titchener: *A Primer of Psychology*, 1898, p. 137.

re-experienced and exerting influence on other conscious processes. Such experiences for some reason made such an initial impression, that the resulting tendency persists for years, perhaps for life. They belong rather to the second level of habit.

Habits arising from mental experiences of unusual intensity or interest, form the second level. They are of more than usual persistence and influence on consciousness. Eating chicken is not an uncommon occurrence, and one would scarcely expect it to give rise to a persisting tendency. I ate some at a restaurant a few years ago, however, which made me ill, and I not only have a vivid recollection of that particular experience, but the tendency resulting from it still occasionally affects other conscious processes, as in making me view with suspicion chicken on a bill of fare. I have a friend whose brother was killed in the Spanish war. The habitual tendency resulting from the grief and the shock of the news persists so strongly that the sight of the national colors is enough to recall the tragic death of the brother. The memory of that single experience five years ago will doubtless be subject to re-experience throughout his life; and at the same time, the tendency making possible its recall; will influence other parts of consciousness, as his attitude toward war and international arbitration. We seem justified, then, in placing in a separate class, which we call the second level of habit, all those habitual tendencies which arise from mental experiences of extraordinary intensity, and which show an unusual degree of persistence and of influence on consciousness.

A third level of habit includes those tendencies to habitual functioning which grow up about the more or less related experiences of one's profession or employment. This class of habits is characterized as arising in adult life, rather than in youth, and as being based on the habitual round of daily activities. The definite limits of the "technical memory" and its high efficiency within those limits, professional ways of thinking, the fact that the predominant interests are those of the profession,—all point to the existence of strong tendencies arising from the habitual activities of one's occupation. Some are specific habits, as those which keep ready the body of definite facts involved in the profession; others are general, as those which shape new mental experiences in the mould of the professional mind. One finds illustrations in the technical knowledge of the engineer, the habits of thought of the lawyer and the mannerisms of the doctor. Every individual is subject to habits derived from the past experiences of his position in life and his round of daily duties, which on one hand determine specifically his knowledge, manual skill, etc., and on the other, as general habits, show themselves in peculiarities of

thought, personal bearing, attitude toward others, and in countless other ways.

The fourth level of habit includes those habits resulting from breeding, education, and early training, in short, the activities and environment of childhood. They manifest themselves in differences of speech, as between a New Englander and a Southerner; in matters of personal attitude, as politeness and courtesy; in such habits as concentrated attention, carefulness, and diligence; in manner of dress and care of the person; and in all those habitual ways of reacting which are fixed before one is twenty-five, and thereafter hold one in an unrelaxing, life-long grip. These tendencies which take shape in the plastic period of childhood, are more deeply seated than the habituations previously mentioned, and exert a stronger influence on consciousness. It is hard to over-emphasize the importance of their influence on the individual. They differentiate the city boy and his country cousin; the product of the slums, and the scion of aristocracy. When these tendencies have once taken form, it is almost impossible to eradicate them—the college boy who puts on the shop-clothes of the mechanic, is still recognizable despite his disguise; he will read a paper while his companions loaf through the noon hour, and "wash-up" at night with a care for cleanliness to which they are unaccustomed—at every turn the habits formed in youth shape his present. The Indian boy taken from a wigwam and sent through Carlisle or Hampton, in many instances, after graduation goes back to the wigwam; the tendencies of his earliest years could not be overcome by later training.

The groups of habitual tendencies so far discussed are alike in that all the tendencies concerned result from mental functioning during the lifetime of the experiencing individual. As such, they are called "acquired" tendencies. In the ordinary acceptance of the term habit, it includes only these acquired ways of functioning. It is convenient, however, for certain purposes,—and it can hardly be confusing,—to treat with the foregoing levels of habit, a fifth level which shall include all those tendencies which are innate in the individual and are hence termed "natural" or "hereditary." Every normal child is born with a tendency to talk, and with various instincts, as acquisitiveness and curiosity. In addition, there are hereditary tendencies which vary more distinctly with the individual. We say one child is naturally musical, another stupid, another sympathetic, and so on. By this we mean that over and above the shaping influences of environment, heredity determines in part at least the characteristics of the individual. And as far as the mental side of the nature is

concerned, we mean by this hereditary endowment, that there are in every individual innate tendencies, or functional ways of least resistance in the physiological substrate of mind. These hereditary leanings are the deepest seated of all our tendencies, and the most far-reaching in their influence on consciousness. The artist is born, not made, we say; and so each individual has definite predispositions given him at birth which fit him for this or that vocation, and determine to an extent which it is impossible to define exactly, his mental possibilities, temperament, and the general course and character of his after-life. It is difficult to tell just where these hereditary tendencies end and where those acquired through early training and education begin, for in a definite case it may be impossible to trace back a tendency noticed in adult life. The distinction betweeen the two, however, is theoretically clear: acquired tendencies, habits proper, result from mental functioning in the life of the individual; hereditary tendencies are innate. The last class of tendencies manifest themselves more commonly in determining the general form of consciousness; though, in instincts, they may give rise to specific conscious processes. Acquired habitual tendencies show themselves in the one way, or the other, or in both ways.

Summarizing this section, we may say that habit manifests itself in consciousness in two ways, in the reappearance of specific processes experienced in the past, and in shaping new processes in accord with past experiences. So we have specific or content habits, and general or form habits. Further, the classification of habits according to levels, indicates their origin and roughly, at least, the degree of their persistence and influence on consciousness.

The Development of Habit.

The last main division of our enquiry is concerning the development of habit. In explaining what habits are and how they come into existence, we are transferred at once from a psychological to a physiological standpoint. Modern psychology holds as a primary postulate that every mental process is accompanied by a characteristic neural process in the central nervous organs. The explanation of habit is that the passage of a particular nervous excitation, or series of excitations, through these central organs leaves them disposed physiologically for a repetition of the same nervous process. If a mental process is experienced, the passage of the corresponding nervous processes through the brain leaves that organ more ready to function with the same nervous processes again,—hence the correlated mental process is likely to reappear in consciousness. The nature of the nervous excitation is still undetermined; if

we assume that it involves some sort of molecular rearrangement in the path it follows, as seems likely, there is open to us the vivid explanation of habit which James and others give. The excitation leaves a trace or path in the nervous structure, which repetitions of the excitation deepen and widen, as running water digs its channel. The path is maintained permanently by the processes of nutrition, which follow the lines of molecular rearrangement. The persisting path constitutes the tendency to the repetition of the nervous discharge which caused it, and hence to the reappearance of the correlated mental experience. At any rate, whether or not we think in terms of actual "paths" and "canals," we shall find it, at least, necessary to assume that a nervous excitation leaves some structural change which disposes the brain to the reproduction of the same process; and that this structural trace, as we have said, provides, on the mental side, the disposition toward the reappearance of experiences formerly in consciousness. The development of a habit, therefore, means the development of a physiological modification, or tendency.

Assuming this to be the nature of habit, we shall consider the conditions favorable to its development, and correlate them with physiological changes.

1. The habitual tendency favoring the reappearance of mental processes gains strength by repetition of the processes. One learns a poem by repeating its words and thinking through its thoughts again and again. One acquires a muscular adjustment by repeating the movements; and a habit of reflection is secured by repeated processes of reflection before action. The fact is universally admitted that repetition or practice is the great factor in acquiring a habit. This is exactly what a physiological explanation of habit would expect. The first passage of the nervous excitation would leave only a slight trace, perhaps; but repetition of the same excitation would wear the path deeper. The physiological explanation, therefore, furnishes precisely the conditions demanded by the fact that a habit grows strong by repetition of the conscious processes involved.

2. A mental experience of great intensity or interest results, without repetition, in a strong habitual tendency. The case of my friend who lost a brother in the war is in point here. The intense mental experience is doubtless represented on the physiological side by a nervous discharge of great intensity. This, in a single experience, would leave a trace or path of as great "depth" as one resulting from the repetition of a weaker excitation. So, a great flood in a single day tears out a path, which a smaller stream would require years to form.

3. The fact that processes attended to produce habitual

tendencies, while those not attended-to, do not, must be considered. If I give my whole mind to learning a new set of movements, the habituation is much more rapid. The things remembered are the things which were attended to when experienced. The mental processes which later affect the course of consciousness, *e. g.*, the unusual weather conditions which made me weather-minded for a day or two, are similarly the experiences which are attended to. In general, then, if one attends to a mental experience, it gains the possibility of recall and the power of influencing other processes in consciousness; that is, the mental process which is attended to produces an habitual tendency. In Titchener's phrase, "Habit means foregone attention." Neurologically, it seems most probable that attention involves an inhibition of all nervous excitations except those underlying the mental process attended-to; these last are facilitated in their passage through the cortical centers. This gives the nervous excitations underlying the mental process attended-to, a relatively great intensity, though no absolute increase in intensity be assumed; only mental processes attended to, therefore, are represented by neural processes of sufficient intensity to leave a trace in the cortex.

4. The physiological explanation covers, too, facts of mental constitution which are best explained by referring them back to heredity. If a lawyer is legally-minded, the obvious explanation is that the experiencing of great masses of legal mental processes has built up neural paths of tendencies, which determine the form his present mental processes show. The action of the new-born infant in taking food, and its display of curiosity, however, cannot be similarly explained as the result of previous functioning on its own part. The conception of physiological tendencies covers such cases, by assuming innate nervous conditions which provide for certain reactions; conditions similar in influence to those which the individual builds up by mental functioning. So, we get our conception of hereditary and acquired tendencies, which while functioning in the same way, have a different origin. The former are innate modifications of the central nervous organs; the latter are modifications brought about by functioning.

5. Another fact to be considered is the easy formation of habits during childhood. In our discussion of the levels of habit, we noted that the strongest of our acquired tendencies arise during youth. If tendencies have a physiological basis, this would be expected. Growth and the nutritive processes generally, are most active in youth; and the bodily organs are extremely plastic at that period in life. This is especially true of the nervous system. Cortical traces produced by nervous excitations would therefore be deeper in childhood than in adult

life after the nervous organs have "set." Hence childhood would be, as it is, the great period of habit-formation. The physiological explanation fits the conditions exactly.

We may conclude that the development of a habit means the development of modifications, or tendencies, in the nervous system. These neurological changes are occasioned in the central nervous organs by the excitations corresponding to the mental processes of the habit. The chief factors favoring the development of habit are repetition of the mental processes involved, attention to them, the intensity of the experience, and plasticity of the nervous system. These factors all contribute to build up the paths of discharge which are followed by the neural processes underlying the habituated mental experience. The existence of such a path, or in other terms, the presence of the habituation tendency, is the physiological basis of habitual functioning.

General Summary.

This paper has attempted to consider certain phases of the great mass of mental life termed habitual. Habit was defined as the mode of mental functioning in which repeated processes are in consciousness; a habit as the functioning, or form of consciousness, involving a particular repeated experience. Habits, as "modes of functioning," or "forms of consciousness," are not in themselves conscious; a study of habit in consciousness, therefore, is a study of the habitual conscious processes in which habit expresses itself. Heretofore, habit has been treated chiefly from the functional standpoint, in its relation to the individual and with objective reference. In this paper, we have discussed three problems with regard to habit:

1. A statement of the structural differences between the habitual and non-habitual states of consciousness. There are differences in pattern, or arrangement of processes, as follows: The processes in the habitual state are meager, uniformly indistinct, and fused into an associated series; those in the non-habitual state bulk large, are of two degrees of clearness, distinct and indistinct, and are consciously joined together by attentive selection and rejection. Further, the habitual consciousness has as a distinguishing process, the mood of familiarity with partly habituated experiences, and of indifferent of-courseness with those completely habituated. The non-habitual consciousness has as its characteristic process, the experience of directed effort. Feeling processess are relatively strong in non-habitual functioning, and pass to indifference under habituation.

2. A classification of habits. Habits are of two classes: specific habits, which provide for the re-experiencing of past

processes; and general habits which shape present processes in accord with past experiences. Further, habits may be classified according to four or five levels: (a) those depending upon *recency* of occurrence alone for their persistence; (b) those due to mental experiences of great *intensity*; (c) those arising from the professional or other habitual daily activities of adult life, and hence referable to *recency* of occurrence and *repetition;* (d) those originating in training and other influences during childhood, and therefore, due to factors of *intensity* and *repetition;* and (e), if we may pass beyond the acquired tendencies, innate tendencies which manifest themselves like habitual tendencies in their influence on consciousness, and which may be tentatively regarded as the resultant of all the factors of habituation in some way racially summated. The order of levels indicates roughly the degree of persistence and of influence on consciousness of the habits in the various groups. Instinctive habits are strongest. Habits which exist simply through recency of occurrence are, other things equal, the weakest.

3. The development of habit. Habit involves neural modifications, or tendencies, which are brought about, save such as are hereditary, by the neural excitations underlying habitual mental processes. The important conditions favoring the development of habit, are repetition, attention, intensity of the experience, and plasticity of the nervous system.

THE INFLUENCE OF ACCOMMODATION AND CONVERGENCE UPON THE PERCEPTION OF DEPTH.

By J. W. BAIRD, B. A., Ph. D.

TABLE OF CONTENTS.

PAGE.

Introduction.
I. Historical, 151
II. Experimental, 170
i. Description of Apparatus, 170
ii. Description of Method, 173
1. With abrupt change of distance, 173
2. With gradual change of distance, 174
iii. Results.
A. Monocular Abrupt Series, 176
B. Binocular Abrupt Series, 177
C. Monocular Gradual Series, 177
D. Monocular Estimation of Absolute Distance, 183
E. Binocular Estimation of Absolute Distance, 183
III. Discussion of Results.
A. Monocular Abrupt Series, 183
B. Binocular Abrupt Series, 191
C. Monocular Gradual Series, 191
D. Monocular Estimation of Absolute Distance, 194
E. Binocular Estimation of Absolute Distance, 194
IV. Interpretation of Results, 194
V. Theory, 197

The present study is an attempt at the settlement of a much-disputed question: the question as to what part the mechanisms of accommodation and convergence play in our visual perception of the third dimension. For many years past, psychological opinion as to this point has been sharply divided; and the experimental data hitherto available are inadequate to a final decision of the question. The evidence adduced in the following pages points unmistakably to some such theory of visual space perception as is advocated by Wundt. A theory of space perception at large must of course be confirmed along many converging lines; and the present investigation is concerned only with a single partial problem. So far as they go, however, our results speak unequivocally for the influence of movement factors upon the visual perception of depth.

The writer's attention was first directed to the general problem of visual space perception in 1899, when he was a student of psychology in the University of Wisconsin. From 1899 to 1901 he was engaged, though with frequent interruptions,

upon a review of the literature of psychological space. Experiments upon the special problem of this paper were carried out in the Cornell Psychological Laboratory during the academic year 1902-3.

The study falls naturally, therefore, into two parts; an historical survey of the course of development of psychological doctrine regarding the visual perception of the third dimension,[1] and an account of the experimental work upon which our own conclusions are based.

I. Historical.

Attempts were made as early as the fifteenth century to determine the factors contributing to the perception of depth. Da Vinci[2] in his Treatise on Painting described various devices employed by painters for the production of the illusion of relievo and distance. It is impossible to determine who first advanced the theory that the adjustment of the ocular mechanisms, in convergence and accommodation, furnishes a criterion of distance. It is evident, however, that this theory had been advocated as early as the beginning of the seventeenth century. Aguilonius[3] mentions the theory as current in his day and attempts to refute it. In the opinion of Descartes[4] both the accommodation of the refractive media of the eye, and the convergence of the visual axes contribute to the perception of distance. "The perception of distance . . . . depends primarily upon the form of the eyeball, for its form when we see a near object must be slightly different from its form when we see a more distant object. And, according as we bring about in the form of the eyeball a change which is appropriate to the distance of the object seen, we also change

[1] Much of the literature quoted below has important bearings upon the question of space perception in general. It has been our aim, however, to draw upon it only in so far as it has reference to our special problem. We hope that the reader will overlook what might otherwise seem to be serious omissions in our historical sketch.

[2] Leonardo da Vinci: 1452-1519, *Trattato della Pittura*, English translation, *Treatise on Painting*, by J. F. Rigaud. New Ed., 1877. §§ 117, 121, 124, 178, 187, 191, 199, 204-211, 283-324, 340-344, 348. It is interesting to note that, although da Vinci's analysis of the perception is far from being complete and exhaustive in the light of modern science, he yet enumerates and describes practically all of the factors employed even by the modern painter. It is a fact of interest, too, that da Vinci called attention to the influence of binocular vision upon the perception of depth. He just missed stating the principle of disparity of retinal images which Wheatstone discovered more than three hundred years later. *Treatise on Painting*, § 124; Charles Wheatstone, *Philosophical Transactions*, 1838. pp. 371-394.

[3] Francisci Aguilonii: *Opticorum libri sex*. Antwerp, 1613. Lib. III. Prop. III.

[4] Cartesius: *Dioptrice*. 1637. VI, 11 and 13.

a certain part of our brain in such manner as is instituted by nature to make the mind perceive that distance. . . . . Secondly we perceive distance by the relation which the two eyes bear to each other. For consider a blind man, who holds in his hands two converging staffs. Their length he does not know, but knows only the distance between his hands, and the magnitude of the angles which the converging staffs make with an imaginary line joining his two hands. He perceives by a sort of natural geometry (*ex geometria quadam omnibus innata*) the distance of the point of intersection of the staffs. So we, when our eyes are converged upon a point, know the distance of that point by the length of the interocular line and the magnitude of the angles formed at the points of intersection of the interocular line and the visual axes."[1]

Descartes' theory of the part played by accommodation lapsed with the lapse of the theory of the mechanism of accommodation which it assumed,—although it is not different in principle from the view dominant in the psychology of a few decades ago. His explanation of the function of convergence was rejected by Berkeley who pointed out[2] that as a matter of fact we possess no knowledge of the oculo-geometrical relations whose assumption furnished the basis of the Cartesian account.

Malebranche[3] enumerates six "signs of distance." The first and second of these are, however, nothing more than Descartes' accommodation and convergence criteria, and the other four do not concern us here. Molyneux[4] defends the Cartesian theory against the attacks of opponents and brings forward another criterion of distance which has, however, never found favor. He is of opinion that "when we estimate the distance of nigh objects, either we take the help of both eyes, or else we consider the pupil of one eye as having breadth, and receiving a parcel of rays from each radiating point. And, according to the various inclinations of the rays from one point, on the various parts of the pupil, we make our estimate of the distance of the object."[5]

Berkeley[6] set himself the task of showing "the manner in which we perceive by sight the distance . . . . . of objects. . . . . It is, I think, agreed by all that distance of itself, and immediately, cannot be seen. For distance being a line directed endwise to the eye, it projects only one point in

[1] Cartesius: *Ibid.* VI, 11, 13. Paraphrase.
[2] G. Berkeley: *An Essay towards a New Theory of Vision*, 1709, § § 4, 8-17.
[3] N. Malebranche: *Recherche de la Vérité*, 1675, I. 9.
[4] Wm. Molyneux: *A Treatise on Dioptrics*. Dublin, 1690.
[5] *Ibid.* Part I.
[6] *Op. cit.*, § § 1 and 2.

the fund of the eye—which point remains invariably the same, whether the distance be longer or shorter." He refutes the contentions of Descartes and of Molyneux[1] and maintains that accommodation and convergence furnish a threefold "sign" of the distance of near objects. "First, it is certain by experience, that when we look at a near object with both eyes, according as it approaches or recedes from us, we alter the disposition of our eyes by lessening or widening the interval between the pupils. This disposition or turn of the eyes is attended with a sensation, which seems to me to be that which in this case brings the idea of greater or lesser distance into the mind. . . . . Secondly, an object placed at a certain distance from the eye, to which the breadth of the pupil bears a considerable proportion, being made to approach, is seen more confusedly. And the nearer it is brought the more confused appearance it makes. And, this being found constantly to be so, there arises in the mind an habitual connection between the several degrees of confusion and distance; the greater confusion still implying the lesser distance and the lesser confusion the greater distance of the object. . . . . Thirdly, an object being placed at the distance above specified, and brought nearer to the eye, we may nevertheless prevent, at least for some time, the appearance's growing more confused, by straining the eye. In which case that sensation supplies the place of confused vision, in aiding the mind to judge of the distance of the object; it being esteemed so much the nearer by how much the effort or straining of the eye in order to distinct vision is greater."[2]

Berkeley's Essay was followed by a voluminous criticism—favorable and adverse; but little positive advance was made until the beginning of the last century. Berkeley's theory was opposed by R. Smith,[3] Condillac,[4] Wm. Porterfield[5] and others, but was supported by Condillac,[6] Voltaire,[7] D. Hartley,[8] T. Reid,[9] Adam Smith,[10] Dugald Stewart,[11] Thomas Brown,[12] Sir Wm. Hamilton,[13] A. Bain,[14] John Stuart Mill,[15] and J. Mackintosh.[16]

[1] *Ibid.*, § § 4-15 and *Appendix to Second Edition.*
[2] *Ibid.*, § § 16-27.
[3] *A Complete System of Opticks*, Cambridge, 1738.
[4] Essais sur l'origine des connaissances humaines, 1746, I.
[5] *A Treatise on the Eye, the Manner and Phenomena of Vision*, 1759.
[6] *Traité des Sensations*, 1754. Condillac was at first opposed to the Berkeleian theory of vision but later accepted it.
[7] *Élémens de la philosophie de Newton*, 1738.
[8] *Observations on Man*, 1749.
[9] *Inquiry into the Human Mind*, 1763.
[10] *Essays. On the External Senses*, 1795.
[11] *Elements of the Philosophy of the Human Mind*, 1792.
[12] *Lectures*, 1811.
[13] *Lectures on Metaphysics*, 1837; *Reid's Works*, 1847.
[14] *Senses and Intellect*, 1855.
[15] *Discussion*, 1859. [16] *Dissertations*, 1862.

The Berkeleian principle, in so far at least as it concerned convergence, was taken up and extended by Steinbuch in Germany.[1] Steinbuch asserted, of all three dimensions of space, what Berkeley had posited for depth alone,—that their perception is a result of the sensations aroused by the contraction of the ocular muscles. When an object makes its appearance in the field of vision, we run our eyes over its various dimensions; and the amount of spatial extension which we ascribe to it is determined by our consciousness of the amount of muscular contraction employed in successively regarding its several parts.

A novel explanation of the *modus operandi* of depth vision was brought forward by Lehot.[2] In the opinion of Lehot the retinal image of a solid object itself possesses tridimensionality. This corporeal image, standing upon the retina, projects into the substance of the vitreous humor, and is there sensed as tridimensional by means of the sensitive fibres with which that humor is supplied.

Hueck[3] was the first investigator to attack the problem experimentally. He performed a series of experiments which he thought established the fact that ocular movements of convergence influence our judgment of the position and distance of the objects fixated. Besides establishing this relation for the normal eye, Hueck maintained that certain abnormal phenomena in space localization were explicable from the pathological condition of the ocular muscles. A few years later, Meyer[4] described experiments which seemed to prove that consciousness of change of convergence of the visual axes gives rise to a change in our estimation of the magnitude and distance of visual objects. Meyer's experiments consisted in a successive fixation of points before and behind a surface upon which was printed a series of congruent figures—wall-paper patterns. He found that with near fixation, or increased convergence, the figures appeared to be small and near, while with decreased convergence they seemed to be larger and more remote. In a later and more detailed investigation, Meyer[5]

[1] *Beiträge zur Physiologie der Sinne.* Nürnberg, 1811.

[2] C. J. Lehot: *Nouvelle théorie de la vision.* Paris, 1823. This is the earliest record we have been able to find of an attempt to explain depth-vision from a purely Nativistic standpoint. Lehot's treatise antedates the work of Johannes Müller—the reputed founder of Nativism—by several years. Moreover Müller's *Vergleichende Physiologie* is, after all, only the modest beginning of a very modest Nativistic theory. It contains no more than the germ of the radical Nativism of his successors.

[3] Alex. Hueck: *Die Achsendrehung des Auges*, Dorpat, 1838.

[4] Hermann Meyer: *Ueber einige Täuschungen in der Entfernung. Archiv für physiologische Heilkunde*, 1842.

[5] Hermann Meyer: *Ueber die Schätzung der Grösse und Entfernung. Poggendorff's Annalen der Physik und Chemie.* III, *Reihe.* XXV, 1852. 198-207.

employed a modified form of the Wheatstone stereoscope. He found that, if two figures were inserted in the slides and combined into a single image, this stereoscopic image appeared to approach and recede from the observer, in the direction of depth, according as the figures were moved back and forth in the slides. He showed by means of a drawing, representing the paths of the reflected rays, that the change of apparent distance corresponded throughout the series with the change of convergence of the visual axes. That is, if with any given position of the figures in the slides, the eyes were required to assume a position of greater convergence in order to combine the images, the single image was projected to a near distance, while, on the other hand, if a farther point must be fixated in order to bring about the blending of the images, the visual object was localized at a greater distance from the eye.

In 1852 Lotze brought forward his well-known theory of "Local Signs."[1] When an object makes its appearance in the field of vision the eyes reflexly turn in such a manner as to bring the images of the object to the part of clearest vision. The ocular movement necessary for the transference of the image from any lateral point to the fovea differs in magnitude and direction for every lateral retinal point. If now these movements are attended by muscular sensations, and if the latter constitute a series of sufficiently fine gradation, every retinal point will furnish in the peculiar movement which corresponds to it and to it alone, a clue for the localization of its objective stimulus. Nor are actual movements necessary, in the opinion of Lotze; the mere tendency to movement may suffice to bring to consciousness a reproduced sensation corresponding to the appropriate movement, and may thereby furnish the local sign.

The Lotzian doctrine was worked out in detail by Meissner.[2] Meissner assumes that every visual sensation has a breadth, height and depth-value, which are referred to a system of ordinates passing through the point of fixation in external space. The point at which the visual axes intersect the center of this system of ordinates is seen to be the center of the binocular field of vision. This point is the point of reference of the space system; the spatial relations of all other parts of the visual field are evaluated in terms of their distance and direction from this zero-point. The localization of any lateral stimulus in relation to this central point is brought about by the sensation of movement which accompanies the transference of the image to the fovea. Breadth and height-values are furnished in monocular vision; depth-values come to consciousness only in binocular vision.

[1] H. Lotze: *Medicinische Psychologie*, 1852.
[2] Georg Meissner: *Beiträge zur Physiologie des Sehorgans*, 1854.

In the course of its development since the days of Descartes and Berkeley, this Empiristic Theory, as it is called, had not only been filled out in detail; it had been enormously broadened in scope as well. Berkeley had made the perception of depth a function of muscular sensations. The later and more extreme Empirists assert that all space perception—breadth and height as well as depth—is dependent upon sensations arising from eye-movements. Berkeley had further maintained that the relation between a given visual sign and the distance which it signifies is purely arbitrary, and that this relation is discoverable only by experience. That this principle can be posited of breadth and height in the same sense in which Berkeley held it to be true of depth, can scarcely be maintained. It is unfortunate that many Empirists have evaded this issue.

The Empiristic theory did not long remain in undisputed possession of the field. Beginning, as is commonly supposed, with Johannes Müller, there arose the rival theory of Nativism, whose development we must now trace. The terms Empirism and Nativism are unhappily chosen. Neither the Nativist nor the Empirist would subscribe to a rigid formulation of any such doctrine as his title implies. For the latter is as far from holding that our earliest experience is absolutely non-spatial in character as is the former from believing that our adult spatial vision is a connate endowment.

Johannes Müller[1] combats the view advanced by Steinbuch, objecting that the latter's argument is unsound, in that he derives space perception from a consciousness of movements which itself presupposes the idea of space. Müller, on the contrary, believes that the capacity to see space is an innate endowment of the retina. Even when the eyes are closed and unmoved, the retina "sees itself extended." Yet Müller believes that eye movements are an important secondary factor in the development of our adult perception of space.

A new impulse was given to the Nativistic theory by Dove's refutation of Brücke's view. A few years previously, Wheatstone's[2] earliest stereoscopic experiments had convinced their author of the erroneousness of the theory of identical retinal points. It had long been held that the explanation of the circumstance that though we see with two eyes yet we are conscious of but a single image, was to be found in the fact of the paired arrangement of retinal points. Every point on the one retina has its mate on the other; and it is characteristic of

[1] Johannes Müller: *Vergleichende Physiologie des Gesichtsinnes*, 1826, pp. 52 ff.

[2] Charles Wheatstone: *Contributions to the Physiology of Vision*, Philosophical Transactions, 1838, pp. 371-394.

these identical points, that the simultaneous stimulation of any pair gives rise to an unitary perception. Wheatstone, however, found an essential condition of stereoscopic vision in the disparity of retinal images. Since it is impossible that two dissimilar projections of an object can be imaged at the same time upon similar or identical parts of the two retinas, and since, moreover, two dissimilar projections may give rise to a single image of the object, Wheatstone felt himself obliged to reject the theory of identity. But he was also obliged to rest content with this half-way measure; for he was unable to advance a satisfactory hypothesis to replace the old. Brücke,[1] doubtless at the instigation of the horopter discussions in the literature of that period, found a solution of the problem which at once saved the theory of identity and explained the phenomena of stereoscopic vision. Since only those points which lie upon the momentary horopter can at any one instant be seen singly and clearly, Brücke finds it necessary to assume that the normal procedure in seeing a solid object consists essentially in a rapid series of eye-movements, during the course of which the various parts of the object are successively fixated. By this means, the parts are successively imaged upon identical retinal points; and meanwhile the different distances of the parts, *i e.*, the tridimensionality of the object, is perceived from the sensations aroused by the movements of convergence. When we regard stereoscopic pictures, a similar series of eye-movements exposes identical points on the two retinas to the various parts of the object fixated, and the consequent perception of relief is, here too, to be explained from sensations of convergence.

Dove[2] showed that it is possible to get the stereoscopic effect with an instantaneous exposure of the stereograms.[3] Since his exposure was computed to have a duration of less than the ten millionth part of a second, we are left to conclude, either that the eye-movements occur with inconceivable rapidity or that they are not an essential condition of stereoscopic vision. It is a surprising fact that notwithstanding Dove's demonstration, Brücke's theory was still accepted without question by several subsequent writers, *e. g.*, Prevost, Brewster and Abbott.

Panum, however,[4] saw in Dove's demonstration a telling

[1] Ernst Brücke: *Ueber die stereoskopischen Erscheinungen u. s. w. Müller's Archiv f. Anatomie*, 1841, p. 459.

[2] H. A. Dove: *Berichte d. Berliner Akademie*, 1841, p. 252.

[3] This demonstration has since been repeated in various forms by Volkmann, Panum, Donders, Aubert and others. The most familiar modern form is that described by Hering in connection with his falling ball apparatus. *Arch. f. Anat., Physiol. u. wiss. Med., Leipzig*, 1865, p. 512.

[4] P. L. Panum: *Physiologische Untersuchungen über das Sehen mit zwei Augen*, 1858. The writer has been unable to gain access to a

argument against the Empiristic doctrine of psychological space. The theory which he advocates is a Nativism of the most pronounced type. It is true that Johannes Müller had made spatial vision an innate capacity of the retina; but Müller had brought forward only a skeleton theory, and had but vaguely indicated the course of the ontogenetic development of visual space perception which he advocated: Panum attacked the problem more resolutely, and if he did not find a satisfactory solution, at least made a valuable contribution to the literature, and directed the discussion into a path which led his successors to fruitful results. If, as Dove's experiment seemed to show, sensations of movement are a non-essential in spatial vision, it is only natural that Panum should seek to find, in the retina itself, the conditions which are necessary. Accordingly Panum was led back to Müller's hypothesis and himself maintained that visual space is a function solely of the retina and its central nervous mechanism. For Panum the spatial position of a visual object is given us immediately in sensation. This specific sensation of space is conditioned by the position of the image upon the retina. The sensation of depth, which is a product of the simultaneous co-operation of the two retinas he calls "the sensation of binocular parallax." If, now, a characteristic space sensation corresponding to each individual retinal point is aroused simultaneously with the stimulation of that point, and if, moreover, this space-sensation suffices for the localization of the visual sensation at a point in space coincident in all three dimensions with the position of the stimulus, we have a simple and satisfactory solution of the problem. Eye-movements and eye-movement sensations alike become unnecessary and superfluous. But if we ask just what constitutes the differentiation between the several space-sensations belonging to the various retinal points, *i. e.*, if we seek for criteria which may take the place of Lotze's movements and movement tendencies or Steinbuch's "muscle-ideas" in rendering the whole procedure possible or comprehensible, we find that Nativism, even in the hands of Panum, has failed to penetrate the mystery.

The modern form of the Nativistic Theory, as formulated by Hering[1] is, strangely enough, a mixed descendant of the theories of Johannes Müller, Lotze, Meissner, Nagel and Panum. Its assumption of specific space-sensations as constituting the

copy of this work. His knowledge of Panum's position has been obtained at second-hand from the numerous discussions in the literature.

[1] Ewald Hering: *Beiträge zur Physiologie*, 1861-4; *Die Lehre vom binocularen Sehen*, 1868; *Raumsinn des Auges*, in Hermann's Handbuch der Physiologie, 1879, III, p. 343.

local coloring of the visual image is a characteristic of the theories of Müller and Panum; its nomenclature had already been given to the world by Meissner, and its advocacy of the dependence of the space-value (or local sign) upon distance and direction from the retinal meridians is also found in Lotze and Meissner. However, it must be recognized that Hering has worked out the Nativistic theory with greater detail than any of his predecessors; and if in its present form the theory has suffered most from hostile criticism, this is doubtless due to the fact that the modern form can be more clearly envisaged and its inherent nativistic limitations more clearly seen.

The theories of Lotze and of Hering have in common the assumption that to every retinal point belongs its own space-value; and in both theories this space-value is a function of the distance and direction of the point in question from the *fovea centralis*. But whereas Lotze assumes that the space-values (local signs) come to consciousness *indirectly*, *i. e.*, through the mediation of the sensation aroused by the movement required to transfer the image from that point to the fovea, Hering assumes that space-values come *directly* to consciousness, as sensations in their own right. To Hering, then, the stimulation of any retinal point gives rise to a two-fold sensation: a light sensation, whose attributes are essentially determined by the character of the objective stimulus, and a space-feeling, whose nature is determined by the position of the retinal point stimulated. Nagel had called attention to the familiar fact that the position of a point in external space is determined when we have determined its direction and distance from the fovea. But whereas Nagel derived our determination of these two spatial relations from sense-data,[1] Hering posits specific retinal sensations of direction and depth. The former he again subdivides into feelings of breadth and of height; so that the space-feeling which arises on the stimulation of any retinal point has a three-fold content: a feeling of breadth, of height and of depth. The fovea, as for Lotze and Meissner, is the central point or point of reference of the whole space-system, and itself possesses a space-value = o. The space-value which accrues to an individual retinal point, in virtue of the space-feelings aroused on the stimulation of the point, are not absolute but relative only. Hering's space-feelings therefore provide merely for relative space-localization,—localization in relation to a given nuclear point of visual space. This nuclear point (*Kernpunkt*) coincides approximately with the momentary point of fixation.

This theory makes no use of eye-movements in its account of the origin of spatial vision. Indeed, Hering explicitly

[1] A. Nagel: *Das Sehen mit zwei Augen*, 1861, pp. 178 ff.

asserts that, in binocular vision at least, eye-movements are the effect, not the cause, of the perception of depth.[1]

Moreover, since the seeing of depth is, for Hering, a function of the relative position of the images upon the two retinas, it is evident that his theory is unable to explain the monocular estimation of depth. Since Hering follows Panum in making depth vision a product of the simultaneous co-operation of the two retinas, he is forced either to deny to monocular vision the possibility of estimating depth, or to eke out his theory by supplementary hypotheses in order to account for this possibility. Of the two evils he chooses the latter; but in so doing he is forced to the damaging admission that the difference between monocular and binocular depth estimation is not one of degree only, but is an essential and absolute difference. The latter is immediate, while the former is mediated by accommodation;[2] they are as distinct from each other as are the processes of sensation and of inference.

Besides the Nativistic and Empiristic theories discussed above, there has also been advanced a Genetic theory of visual space. The latter is an off-shoot from the Empiristic stem; its origin may be traced to a series of experiments performed by Wundt during the years 1858 to 1862.[3] The object of Wundt's investigation was to determine the influence of accommodation and convergence upon the perception of depth. In his experiments, a fine black silk thread was suspended vertically at a point between an observation-tube and a distant white background. Experiments were made in both binocular and monocular vision, the same method being employed throughout both series. The observer looked through the tube, and, after fixating the thread in its first position, turned his head while the thread was being moved to a new position. He again looked through the tube, fixated the distant background, then refixated the thread, and judged whether the movement of the latter had been in the direction of approach or recession. In this way were determined the limens of approach and recession for a series of distances ranging from 40 cm. to 250 cm. The monocular experiments yielded two general results: (1) The observer was unable to form any definite estimate of absolute distance, but could perceive changes of distance within certain limits; (2) the limens of recession were invariably greater than those of approach. The binocular experiments showed less indefiniteness in the estimation of absolute distan-

[1] *Beiträge*, p. 320.
[2] *Raumsinn des Auges*, p. 546.
[3] *Zeitschrift für rationelle Medicin von Henle und Pfeufer*. These papers were collected and published under the title, *Beiträge zur Theorie der Sinneswahrnehmung*, 1862.

ces, though the estimate invariably fell short of the actual distance (ranging from ⅓ to ½ of the actual distance). Here, too, however, differences of distance were correctly estimated within certain limits, and these limits were considerably narrower than those in monocular vision. Indeed, the results indicated a 2.5 to 4.5 times greater sensitivity to depth in binocular vision. The difference between the limens of approach and recession vanished in the binocular series. Wundt concluded that, in the latter series, the comparison of two distances was a comparison of two convergence positions by means of muscular sensations, and that the perception of distance was essentially conditioned by this sensation factor. He put a different interpretation, however, upon the monocular results. Since accommodation from a farther to a nearer point is accomplished by a muscle contraction, and may, therefore, be assumed to be attended by a muscular sensation, while accommodation from a nearer to a farther point is accomplished by a release of muscle contraction, and cannot, therefore, give rise to any muscular sensation, Wundt concluded that accommodation is capable of contributing to the perception of change of distance only in the direction of approach. He was of the opinion that, in his monocular experiments, the perception of the recession of the thread was rendered possible solely by the change in the size of the visual angle subtended by the receding thread.

The net results, then, of Wundt's investigation are his conclusions that in binocular vision the perception of approach and recession is the resultant of sensations of convergence, while in monocular vision the perception of approach alone is the resultant of sensations of accommodation.

Wundt is essentially an orthodox Empirist in his interpretation of these results. In the opinion of the earlier advocates of Empirism, our knowledge of spatial relation is the product of a distinctly conscious process. The 'visual sign' is present in consciousness as such; the judgment of the distance signified in a conscious inference. The substitution of an unconscious inference made its appearance in the later forms of the theory. This was especially characteristic of the view of Helmholtz and was characteristic also of Wundt's earlier theory. "With the accommodation is associated a feeling in the eye from which an inference is drawn as to the approach of the observed object." [1]

The Scottish school assumed a process of association to explain the origin of visual ideas of space. Thus, in the system of Bain, the association of retinal sensations with sensations of

[1] Wundt: *Beiträge*, p. 109.

different intensity arising from ocular movements, gives rise to ideas of linear and two-dimensional space. The perception of depth is traced to an association of retinal sensations with sensations of convergence and accommodation.

In his later writings on psychological space Wundt modifies his earlier view. His theory of Complex Local Signs assumes a characteristic psychical process to account for the perception of space.

Meanwhile the experimental investigation was continued with renewed vigor. Helmholtz[1] describes an illusion which shows the influence of accommodation upon the estimation of distance. "At the end of a tube, blackened on the inside, I set up a screen pierced by two vertical slits which were covered, the one with a red and the other with a blue glass. A noticeably greater effort of accommodation was required to see the red line distinctly than was the case with the blue. Finally after numerous trials I got the impression that the red line stood out nearer than the blue."

In 1894 Hillebrand contributed a paper to the discussion.[2] Hillebrand objected that binocular experiments are inadequate to a solution of the problem since the use of two eyes introduces secondary criteria, notably crossed and uncrossed double images, and thus prevents an isolation of the influence of the factors of accommodation and convergence. His experiments dealt with monocular vision alone. They were concerned chiefly with an investigation of the relation between accommodation and depth localization; but, since accommodation and convergence are intimately associated physiologically, the influence of convergence was not excluded. He also objected to the employment of a thread as fixation-object, since the change in its apparent diameter with change of distance and inequalities in the thread itself may furnish a clue to distance estimation, and thus defeat the end of the investigation. In Hillebrand's apparatus the clean-cut edge of a black cardboard screen was brought into the median plane of the field of vision,—the screen thus hiding half of the distant white background. The mathematical line which marked the boundary between the black and the white halves of the field of vision served as fixation-object. A mechanical device enabled him to expose this fixation-object at any distance (up to one meter) from the observer, to move it gradually towards or from the observer, and to remove it abruptly from the visual field and to replace it immediately by another similar screen-edge at a

[1] H. Helmholtz: *Phys. Optik*, 1867, 634; 1897, 779.

[2] F. Hillebrand: *Das Verhältniss von Accommodation und Convergenz zur Tiefenlokalization*. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, VII, 1894, p. 98.

different distance.[1] Hillebrand's experiments assumed two general forms. In the first series the screen was gradually moved towards or from the observer, the sliding movement being begun before the screen was exposed. The movement itself was constant and its rapidity was so gauged that the observer could conveniently follow with his accommodation; *i. e.*, the movement was so slow that change of accommodation could readily keep pace with it; dispersion circles were, therefore, never present in any considerable degree. In the second series two screens were employed; after the first had been fixated for a time, it was *abruptly* removed laterally from the field of vision, and the other was immediately brought in at a different distance, the observer being required to state whether in the second case the distance was greater or less than in the first.

When the change of distance was *gradual*, the observers were unable to state the direction of the movement of the fixation-object, although it seems clear that changes of accommodation must have occurred during the movement. From the negative character of the results of this series, Hillebrand concluded that muscular sensations of accommodation either are non-existent, or at least are inoperative in the perception of depth. For this conclusion he finds additional confirmation in the results of his experiments with an Aubert diaphragm. This instrument he fastened to the screen of his apparatus, and in a series of experiments, he found that, if the aperture in the diaphragm was continuously increased or decreased while its distance from the observer remained constant, a distinct impression of recession or approach arose, although the accommodation had meanwhile remained unchanged. When the diaphragm gradually approached, its aperture meanwhile being *rapidly* decreased, there arose an impression of recession, notwithstanding the greater tension of accommodation.

In the series where the distance of the fixation-object was *abruptly* changed, he found distance-limens, before which and beyond which approach and recession were perceived almost without error. These limens are invariably much larger than those of Wundt, ranging from one to two diopter-differences for approach, and from one to four diopter-differences for recession. The excess of recession limens over those of approach which Wundt's monocular experiments showed, was found with only three of Hillebrand's five observers.

How are these correct estimates of relative distance to be

[1] For cut and detailed description of Hillebrand's apparatus, see his paper, *loc. cit.*, p. 108. The apparatus employed in our own experiments was modelled after Hillebrand's. It is figured and described below, p. 170 ff.

explained? Hillebrand admits that the assumption of accommodation-sensations would furnish a satisfactory explanation, were it not for the fact that his observers were unable to estimate gradual changes of distance. In view of this latter fact he considers the existence of accommodation-sensations as exceedingly problematic and their influence as inoperative. He therefore rejects Wundt's theory and casts about for a more satisfactory explanation. From the introspections of his observers he forms a conjecture as to the manner in which any required adjustment of accommodation is made. This conjecture he confirms by experiment and advances as an explanation of his results.

This explanation is as follows: When a point, which is not equidistant with the point of fixation, appears in the field of vision, it images itself upon the retina in dispersion circles. The observer forthwith sets about to effect that adjustment of accommodation which will give rise to clear vision. But he does not know as yet whether greater or less tension of accommodation is the appropriate movement. Either of the two possible movements is chosen at random and deliberately innervated, the effect upon the definition of the retinal image being noted meanwhile. If an improvement in definition follows, the movement is continued until perfect definition results. If, however, the dispersion images are increased by the initial movement, it is abandoned and the opposite movement is chosen and continued until clear vision is attained. Hillebrand performed a series of experiments whose results showed that adjustment of accommodation for a second point requires less time when the observer knows whether it is nearer or farther than the fixation point, than is required when its direction from the fixation point is unknown. This confirms his suspicions as to the manner in which adjustment of accommodation is made, the excess of time required in the case of unknown direction representing the time lost in experimental tests which result in the appropriate movement being finally hit upon. Accordingly, Hillebrand concludes that the conscious impulse of will (*bewusster Willenimpuls*) which innervates accurate adjustment of accommodation from a first to a second fixation point, is the determining factor in the relative depth localization of the second point.

The net results, then, of Hillebrand's investigations are as follows: Muscular sensations of convergence and accommodation do not contribute to depth localization. The adjustment of accommodation from one point to another is accomplished by an impulse of will. And it is the consciousness of this impulse of will which determines the relative depth localization of the second point.

In 1895 Dixon published a paper describing a series of ex-

periments which he had made with a modified form of Hillebrand's apparatus.[1] In the latter's experiments, the normal and the comparative fixation-objects were imaged upon opposite sides of the retina. In order to remove this objection, and also to shorten the time between their exposures, Dixon devised a modification of the original apparatus which exposed both screens at the same side of the field of vision. Dixon finds no essential difference in principle between Hillebrand's gradual and abrupt experiments. Since the latter form seems better adapted to give definite results, he employed the abrupt change of distance almost exclusively in his investigation. His experimental results agree in the main with those of Hillebrand, but his conclusion is widely different. Each of his observers was able to estimate changes of distance correctly but the capacity varied greatly in different individuals. Wundt's observation, that changes from far to near were more accurately perceived than changes from near to far, was not confirmed in every case. Introspection showed that in all observers "the actual criterion of depth was a difference in the rapidity or ease with which the accommodation adjusted itself (or was adjusted by the observer) and not in any conscious direction of the accommodation by the observer." He is not convinced of the soundness of Hillebrand's argument against the muscular sensation theory of depth perception, though he himself makes no use of muscular sensations in accounting for the depth localization of his own observers.

In 1896 Arrer published new experimental data together with an historical and critical discussion of the whole problem.[2] Arrer's apparatus and method differed little from those of Wundt. Indeed, such modifications as were introduced were made at the suggestion of Wundt himself. Threads were employed as fixation-objects; experiments were made in both binocular and monocular vision. Arrer also repeated Hillebrand's experiments with a duplicate of the latter's apparatus and obtained similar results. Two objectionable features of Hillebrand's apparatus were pointed out by Arrer: the juxtaposition of the white and the black portions of the field of vision gives rise to such a degree of contrast and irradiation as renders accurate accommodation uncertain if not impossible. Moreover, the fixation of a mathematical line brings with it no definite idea of its absolute depth, and Arrer is convinced from his investigation that without a definite presentation of abso-

[1] E. T. Dixon, On the Relation of Accommodation and Convergence to our Sense of Depth, *Mind*, N. S., IV, 1895, p. 195.

[2] M. Arrer: *Ueber die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung*, *Phil. Stud.*, XIII, 1896-7, p. 116 ff.; p. 222 ff.

lute distance, perception of relative distance is impossible. Accordingly, Hillebrand's apparatus is rejected as being inadequate to a positive solution of the problem. Arrer defends his binocular experiments against Hillebrand's attack, maintaining that, as a matter of fact, double images were invariably lacking from his experiments, nor did their intentional production furnish anything but a disturbing factor. He also defends the use of threads as fixation-objects. A mathematical determination of the amount of variation of visual angle with approach and recession of the thread in his experiments convinces him that this variation falls below the minimal values which have been determined for the just observable difference of visual angle.

Arrer's monocular experiments gave a somewhat greater liminal value than his binocular, though monocular perception of difference of depth was fairly definite. Wundt's difference of limen for approach and recession was not a characteristic of all observers.

Arrer concluded that Wundt's explanation of the perception of monocular recession is not valid. He ascribes to convergence sensations the leading rôle in depth perception in his monocular as well as in his binocular experiments. The closed eye converges, approximately at least, upon the fixation point, probably being guided in its movement by the accommodation of the seeing eye. This indirect and problematic influence seems to be the sole factor which Arrer conceives to be contributed to depth perception by accommodation.

The net result of Arrer's investigation is his conclusion that the sense-factors of depth-localization, absolute and relative alike, are the sensations of convergence and accommodation. The estimation of depth occurred neither through an immediate perception of the degree of convergence strain, nor through convergence-sensations being experientially associated with the object to be localized. Sensations of convergence and accommodation are, however, those elements in our ideas of space by which reference to depth is conditioned for consciousness.

In 1897, Hillebrand contributed a second paper to the discussion.[1] In view of the fact that the experimental results which he had previously published were subsequently confirmed by Arrer and by Dixon, he finds it unnecessary to bring forward new experimental data. He now reviews the history of the problem and discusses its essential features and immediate bearings upon space theories in general. He restates his objections to the method and apparatus of Wundt and Arrer, criticises

[1] F. Hillebrand: *In Sachen der optischen Tiefenlocalization*, *Zeits. f. Psych.*, XV, 1897, p. 70.

the conclusions of Wundt, Arrer and Dixon, discusses and defends Hering's theory of space, meets the objections raised by his critics and intrenches himself in the position assumed in his previous paper.

In his most recent contribution to the psychology of visual space, Wundt devotes a section of his paper to a discussion of this problem.[1] He meets Hillebrand's objections to his earlier experiments—the influence of double images and change of visual angle—and discusses the possibility of explaining his experiments from convergence alone. He, in turn, objects that Hillebrand's fixation-object was not really a mathematical line, but an indefinite band, shading off from white into black. Accurate accommodation upon this band is impossible, hence the negative character of the results obtained by Hillebrand. It is true that, in the abrupt series, a relative localization of this fixation-object was possible, but there is evidence in the results of Dixon and Arrer that this possibility was due to the presence of secondary criteria of depth. Since these criteria were doubtless introduced because of the absence of normal conditions of accommodation, the inadequacy of Hillebrand's experimental conditions for a positive solution of the problem is evident.

Wundt further maintains that Hillebrand's theory of voluntary innervation does not furnish a satisfactory explanation of depth-localization. The assumption that eye-movements are a pure function of the will is in direct opposition to familiar facts of experience—to the difficulty of isolating a given position of accommodation from its concomitant position of convergence, and to the well known tendency of the eye to turn towards the electric spark in dark-room experiments.

Wundt's and Arrer's results indicate that a much more definite idea of distance is associated with convergence than with accommodation. But just what is the relative contribution of each to the resultant perception of depth must remain for the present an open question.

According to Wundt, the sensations arising from eye-movements are not merely muscular, but are also tendinous and pseudoarticular,—the latter arising from the rotation of the eye-ball in its socket. In small excursions these sensations are not present to consciousness, as such, simply because they fuse immediately with other sensations to form perceptions of space. An analogy is found in the case of arm-movements. When the excursion of arm-movement is small, the movement is still perceived, though the joint sensations are not present as such.

[1] Wundt: *Zur Theorie der visuellen Raumwahrnehmung*, *Phil. Stud.*, XIV, 1898, p. 1, esply. pp. 11-16.

This is but an example of a law that holds for all sense-perception; namely, that sensations of moderate intensity fuse completely to form perceptions. The formation of complex spatial ideas from sensations of ocular movements is, then, but a case of psychical assimilation.

Wundt's present theory[1] is a product of many years' growth. Wundt is of opinion that the idea of space cannot be assumed to arise from light sensations in themselves. The spatial order is the resultant of a combination of sense-data which taken separately possess no spatial attributes. His theory of Complex Local Signs takes its origin from the fact that the eye is at once an organ of vision and of movement. Each of these modes of functioning furnishes a system of local signs; and by local sign Wundt means any datum for consciousness which is effective in the localization of an impression. It is a well-known fact that the same stimulus may arouse qualitatively different sensations at different parts of the retina. Accordingly, Wundt assumes that every sensation possesses its own peculiar local coloring, corresponding to the part of the retina stimulated. These qualitative differences constitute a first system of local signs. Since the movements of the eye are attended by sensations of muscular and orbital origin, they are able to furnish a second system of local signs. Moreover, these two systems are brought into intimate relation by the reflex transference of eccentric impressions to the center of the retina; consequently they may be regarded as a single system of complex local signs. The psychical process which gives rise to the perception of space is a fusion. And it is characteristic of the fusion that the component sense-data are so closely merged in a single product that they are not discernible as sense-data. In the opinion of Wundt, then, eye-movements are an essential factor in the perception of space. Changes of convergence are accompanied by sensations which constitute an important element in the binocular perception of depth; accommodation furnishes sensations which, though assigned a minor rôle, come into play in the monocular perception of depth.

An investigation into the visual perception of space has recently been made by Bourdon.[2] In addition to accommodation and convergence Bourdon mentions, as monocular depth factors, the nodal and pupillary parallaxes and the monocular parallax. The latter refers to changes in the position of the

[1] Wundt: *Logik*, 1880, pp. 437 ff., *Outlines of Psychology*, 2nd. English Edition, 1902, pp. 113 ff., *Grundzüge der physiologischen Psychologie*, 5th Edition, 1903, II, pp. 501 ff.

[2] B. Bourdon: *La perception visuelle de la profondeur*, *Revue philosophique*, 1898, XLVI, pp. 124 ff. *La perception visuelle de l'espace*. Paris, 1902.

eye as a result of movements of the head; by the pupillary and nodal parallaxes are designated changes in the position of the pupil and nodal points relatively to the position of the visual object. Bourdon attaches but slight importance to accommodation as a factor in the monocular perception of depth. Binocular convergence occurs even in monocular vision and furnishes a clue to distance. The pupillary and nodal parallaxes give only a very imperfect knowledge of depth. Of all the possible factors, the monocular parallax is the most important in the monocular perception of distance. Bourdon publishes the results of experiments which he performed with a view to determining the influence of convergence and accommodation in the monocular estimation of distance. In the comparison of two distances—2 m. and 6.5 m.—successively marked off by electric sparks, he found that changes in the direction of approach are much more accurately perceived than changes in the opposite direction. When the two distances were given simultaneously, the results were negative. The absolute estimation of distance is extremely uncertain in monocular vision.

In his first series of binocular experiments two luminous objects appeared successively in the observer's median plane. The standard distance was 1 m. and the comparative distances 1.08 m., 1.12 m. . . . . up to 1.32 m. His results show that for one subject the differential limen was less than .08 while for the other it lay between .16 and .20. In his second series of binocular experiments, he employed distances ranging from 10 m. to 25m.; lanterns at the ends of a dark L-shaped passage-way served as fixation-objects. The observer stood at the intersection of the two arms of the passage-way; after fixating the first lantern he turned on his heel through 90° and fixated the second. The results show a preponderance of right judgments only when the ratio of the two distances was 1:2 or more. An obvious source of error is to be found in the crudeness of the method employed in the second series of binocular experiments. Since the observer was directly opposite a corner of the wall and but a few feet removed from it, there is no guarantee that the first position of convergence and accommodation was not lost and forgotten before the second was assumed. Moreover the most zealous advocates of the influence of convergence and accommodation have never maintained that this influence is operative save in the perception of near distances.

An historical survey of the literature of depth-perception reveals the existence and progressive development of two distinct types of theory.[1] The one dates from Descartes and

[1] Wundt proposes a different classification. *Logik*, pp. 452 ff.

Berkeley and culminates in Helmholtz; the other may be traced from Johannes Müller to Hering. The genetic theory of Wundt occupies, to some degree, a mediating position between the two extremes. Of these three later views, only those of Hering and of Wundt are discutable at the present day. Original as these men are, they represent theories which contain no essential element—save only Wundt's conception of the nature of the space-fusion—which is not considerably older than its modern advocates.

The problem of the visual perception of depth has been before the world for nearly four centuries; yet, though it has engaged the attention of the best scientific minds of that period, in only four instances—Wundt, Hillebrand, Arrer and Dixon—has it been submitted to even an approximately adequate experimental test. If our results are, as we believe they are, decisive, they furnish yet another instance of the celerity and certainty with which the experimental method can deal with disputes of long standing.

The present status of the problem may be summarized as follows:

The only point upon which agreement has been reached is the bare general fact that the motor adjustment of the visual mechanism contributes to depth-localization. As to what is the character of this contribution, and the manner in which it is made, there is, as we have seen, a wide diversity of opinion.

In the opinion of Hillebrand, we must ascribe to accommodation the rôle of furnishing dispersion images, and of occasioning thereby a voluntary impulse to innervate a readjustment of accommodation, appropriate to secure perfect definition. This conscious adjustment of accommodation (or, more properly, the innervation which ushers it in) is the determining factor in the monocular depth-localization of the fixation-point, under the conditions of his experiments.

According to Dixon, the difference in the degree of rapidity or ease with which the readjustment of accommodation came about was the actual criterion of depth in his experiments. According to Wundt and Arrer, changes of convergence and concomitant changes of accommodation furnish muscular sensations which, in turn, constitute the sense-factors of our perceptions of tridimensional space.

II. Experimental.

i. *Apparatus.* The possibility of solving this problem experimentally reduces, in the last analysis, to the possibility of devising a means of isolating the influence of the convergence and accommodation factor, and of determining how, and in

what degree, this factor, unaided by any other depth criterion, is capable of contributing to depth-localization. Hence a fundamental requisite of the apparatus to be employed is that it shall render inoperative all depth criteria save only that of accommodation and convergence. It is, of course, essential that depth-localization by means of accommodation and convergence shall be possible under the conditions of the experiment.

The form of apparatus devised by Hillebrand seems to meet the requirements of the present investigation. The exclusion of binocular vision is the simplest means of eliminating the criterion furnished by disparity of retinal images. The co-operation of the factor arising from change of size of visual angle is most satisfactorily ruled out by choosing for fixation-object an object whose size does not change with change of distance. It is true that Wundt and Arrer have raised serious objections against this apparatus. They found that the influence of irradiation and contrast renders Hillebrand's fixation-object so indefinite and obscure that accurate accommodation upon it was impossible. This may, conceivably, be the case with strong degrees of illumination-intensity of the background. But with moderate intensity of illumination, it is certainly not the case.[1] No one of our five observers, no one of the numerous visitors who interested themselves in the apparatus, ever failed to see the screen-edge as a clear-cut, well-defined line, or experienced the slightest difficulty in accommodating accurately upon it, so long as the screen stood beyond the near-point of accommodation. That this objection is invalid, that accommodation, under Hillebrand's experimental conditions, is possible and practicable, is abundantly proved by the accurate distance-estimates of which our observers were capable.

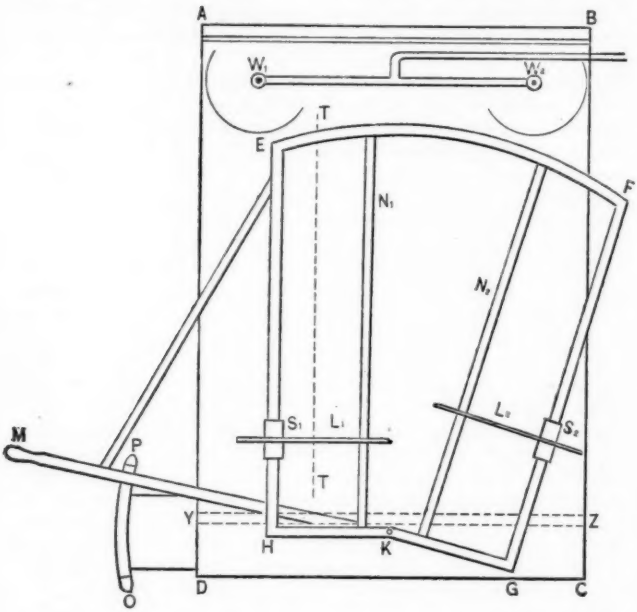
To Hillebrand, then, we are indebted for the apparatus.[2] We have modified it only in non-essential details.

In Figure 1, ABCD is a table 130 cm. wide and 140 cm. long. At AB is erected a vertical sheet of ground opal glass (80 cm. by 100 cm.), which serves as the background. EFGH is a stout wooden frame, pivoted to the table at K, and suspended at E and F from a 25 foot ceiling. On EH and FG are fitted slides, S_1 and S_2, each carrying a vertical screen, L_1 and L_2. A lever, M, is attached to the swinging wooden frame at H, and secured by a brace from E. A wooden arc, OP, is fastened to the side of the table, and provided with padded blocks at O

[1] The intensity of the light reflected to the diaphragm of our observation-tube was equal to the direct light of a standard candle at a distance of 56 cm.

[2] For Hillebrand's figure and description, see his first article, *Zeits.*, VII, 108 ff.

and P. These blocks are so adjusted that when the lever is pressed against O, the edge of the screen L_2 just comes to the longitudinal center of the table, and when the lever is brought up against P, the edge of L_1 is swung to the center of the table.



On the under side of the lever is fitted a padded block, provided with a spring so arranged as to press against the wooden arc, and thus to lock the swinging frame when it has reached the end of its excursion. The suspension of the frame from the ceiling, and the heavy padding of the stop-blocks render the operation of the apparatus noiseless. A shelf-like attachment, fastened to the screen YZ, a few inches above, and parallel to, the table, incloses the part of the apparatus which projects under the screen. The background is illuminated by two Welsbach gas burners, W_1 and W_2, shaded by vertical semi-cylindrical shades of blackened tin. A vertical screen (80 cm. by 100 cm.) is built across the table at YZ. At a convenient height, a brass observation-tube is let into this screen. This tube, which is lined with black velvet, is 40 mm. in diameter and 50 mm. long. Its end farthest from the observer is

provided with a diaphragm containing an aperture 10 mm. wide and 15 mm. high. The tube projects from the screen towards the observer, and is so adjusted that, when the observer is in position, the center of rotation of his eye is vertically over the pivot, K. The inner side of the screen YZ is covered with black velvet to prevent reflection of light to the front of the observation screens, L_1 and L_2. It is also provided with an automatic shutter which closes the diaphragm of the observation-tube. On the outer side of the screen YZ an adjustable chin-rest is fitted. The wooden bars N_1 and N_2 are inserted for the purpose of supporting the bases of the screens L_1 and L_2, thereby ensuring a vertical position of the screen edges.

The most carefully constructed parts of the apparatus are the screens, L_1 and L_2. These screens are 32 cm. wide, 75 cm. high and 8 mm. thick; they are constructed of veneered wood to prevent warping. Along the inner edge of each screen is fitted an L-shaped strip of brass. After the brass had been fastened to the screen, its free edge was carefully worked down by a surface plate to a perfect straight-edge. Then the surface of the screen where the brass and the wood are joined was ground down to an uniform surface, filled with molten wax, painted, sandpapered and repainted until it presented an uniform dead-black surface. The joint was barely perceptible in daylight; it certainly could not be detected in the dark-room where the experiments were performed.

ii. *Method.* The object of these experiments was to determine the limens of approach and recession of the fixation-object for various distances, ranging from 286 mm. (3.5 diopters) to 666 mm. (1.5 diopters). The experiments assumed two general forms: (1) with *abrupt* change of distance, and (2) with *gradual* change of distance.

(*1*) *With abrupt change of distance.* The method employed in this series of experiments was a serial method, without knowledge. The observer seated himself comfortably before the observation-tube; after a pause of from seven to ten minutes for adaptation, he received a signal and brought his eye to the observation-tube. The shutter was opened, and he was asked to assume such a position that half of his field of vision was black and half was white. Then he was required to fixate the screen-edge carefully. When he had secured perfect accommodation upon the fixation-object, he gave a signal; the experimenter grasped the lever, and swung the frame until the lever touched the block at the other end of the wooden arc, thus bringing the second screen-edge to the exact center of the observer's field of vision, but at a different distance.[1] The observer fixated it,

[1] Dixon believes that the interval of time elapsing between the removal of the first screen and the exposure of the second was too

and judged whether it was nearer or farther than the first. Then the shutter was closed, introspections were recorded, and after a brief pause the experiment was continued. The observer was cautioned throughout to adjust his head so that the fixation-object was imaged upon the central vertical meridian of his retina, to move neither his head nor his eye during the observation and to announce the first symptom of fatigue.

In this manner the limens of approach and recession were determined for five distances,—286 mm. (3.5 D), 333 mm. (3.0 D), 400 mm. (2.5 D), 500 mm. (2.0 D) and 666 mm. (1.5 D). Ten liminal values of approach and ten of recession were determined for each of the five distances. The average limens, with the mean variations will be found in the tabulated results.

(*2*) *With gradual change of distance.* A slight modification of the apparatus was found to be necessary for these experiments. The method employed was as follows: the shutter was opened and the observer fixated the screen-edge. When he had acquired a perfect accommodation, he signalled, whereupon the experimenter pushed the screen towards or from him,—the movement being constant and of such rapidity that accommodation could follow it without difficulty. The movement was made by hand, but a considerable degree of constancy and uniformity was soon acquired. The rapidity used in these experiments was approximately 10 cm. in 7 seconds.

It was found early in this series that the sound of the moving slide furnished the observer with a clue to the direction of the movement. Even when the shutter was closed, some of the observers were able, from sound alone, to estimate the direction of the movement with surprising accuracy. Since the presence of this factor would render impossible the isolation of the criteria with which we are concerned, the following plan was devised.

The wooden frame EFGH (Figure 1) was removed from the apparatus. One of the screens, S_1, was taken from its slide and mounted upon a carriage with grooved wheels. A smooth, hardwood track was constructed upon the table (along the dotted line, T) and the carriage set upon it. Our first plan included a pair of parallel tracks upon the table, but it was found impossible to make them sufficiently smooth for our purpose. An almost infinitesimal tremor of the moving car

long and thus tended to render judgments inaccurate and uncertain. We confess that we cannot see any ground for this objection. However, to obtain a measurement of the time required, we arranged an electric contact mechanism, and recorded the time-intervals upon a kymograph. The average duration of the interval (4 series,—each of ten individual tests,—made at different stages during the progress of the experiment) was $1.02 \pm .06$ seconds.

was magnified into a noticeable lateral swing of the screen-edge. The difficulty was overcome by building a second track at the top of the screen. Great care was taken in planning the tracks; the result was a movement of the screen without perceptible tremor and without a trace of noise.

iii. *Results.* The experiments whose results are given below covered a period of time extending from November, 1901, until June, 1902. Each observer gave two one-hour sittings a week throughout that period. The observers were: Dr. I. M. Bentley (*B*), and four graduate students in psychology, Miss B. M. Downes (*D*), Messrs. R. H. Gault, (*G*), M. S. Macdonald (*M*), and H. C. Stevens (*S*).

The apparatus stood in a corner of the dark-room, where it was hidden from view by a curtain. None of the observers —excepting *B*—had any definite knowledge, before the experiments began, of the size or form of the apparatus. Not until the observer had gone into his dark corner and seated himself before the observation-tube was the curtain removed. It was always replaced again before he left his seat at the close of the sitting. So that no observer—with the single exception of *B*—had any extraneous aid in forming his judgments of the distances with which the experiments were concerned.

Observers *G*, *M* and *S* are emmetropic; *D* is hypermetropic (+ 1.18 D.). The distance of the near-point of accommodation of the eyes employed in the monocular experiments were: *B*, 143 mm., *D*, 186 mm., *G*, 132 mm., *M*, 118 mm., and *S*, 123 mm.; no spectacles were worn in any of the experiments. *B's* eyes are defective in that they possess what may perhaps best be described as an unusually great inertia of accommodation. He finds that after accommodating continuously on a near object, he is unable for some time to get a clear image of distant objects. Thus, after reading for an hour or two, he is able only after the lapse of fifteen or twenty minutes to recognize a friend on the opposite side of the street. Whether the difficulty is due to a muscular or lenticular (*i. e.*, decreased plasticity) defect we cannot say. Unfortunately no oculist within reach has been able to diagnose the case, or to make a quantitative determination of the defect. We hope later on to be able to give a more exact characterization of the case.

The experiments fall into five groups: (A) Monocular experiments, in which the change of distance was made abruptly; (B) Binocular experiments in which the distance changed abruptly; (C) Monocular experiments with gradual change of distance; (D) Monocular and (E) Binocular estimations of absolute distance.

Our chief concern will be with the results of group (A). The binocular experiments were made solely for purposes of comparison.

(*A.*) Monocular Abrupt Series.

The object of these experiments was to determine the limens of approach and recession (as defined below) for five standard distances—286 mm. (3.5 D.), 333 mm. (3 D.), 400 mm. (2.5 D.), 500 mm. (2 D.), and 667 mm. (1.5 D.). We employed a serial method, without knowledge, working from equality in both directions. At the beginning of each series, the two screens, L_1 and L_2, were set at any one of the standard distances. Then the shutter at the end of the observation-tube was opened; the observer fixated the edge of L_1 until perfect definition resulted, gave the signal for the movement of the frame EFGH which should swing in the second screen L_2, and then fixated the edge of the latter. He gave his judgment as to the relative distance of L_1 and L_2, and his introspections. For the next exposure, L_2 was set at a different distance from the observer, nearer or farther, and the two screens were successively exposed as before. The series reached its natural conclusion when the observer was sure of the direction in which the second screen had been moved along the slide N_2. The change of distance in each successive setting of the second screen varied from five mm. to ten mm., according to the absolute distance of the standard screen. It remained constant throughout a series.

The nature and conditions of the experiment demand that each series shall proceed from equality to inequality, and never in the opposite direction. An illustration will make this evident. If in the initial exposure of any series the second distance were considerably greater or considerably less than the first, the second screen-edge would appear in dispersion images. Dispersion images would then furnish a criterion of inequality of distance; and so often as they recurred, the observer would be able to say without hesitation that the two distances were unequal. If, now,—as the procedure assumes,—he knew the relative positions of the screens in the initial exposure of the series, and knew also that the series were continuous in one direction, the criterion furnished by dispersion-images would warrant a judgment as to relative distance until equality were almost reached, *i. e.*, until the dispersion-images vanished from the series. Hence in an experimental series proceeding from inequality, judgments of relative distance would be possible without recourse to changes of accommodation and convergence.

We have, accordingly, in our experiments, proceeded only from equality. It is impossible that dispersion images could have been a contributing factor in the results tabulated below. For while dispersion images constitute a criterion of inequality of distance, they are powerless, at least under the experimental

conditions of the present investigation, to furnish evidence of the direction of inequality. They arise when the second point of fixation is nearer *or* farther than the first; they are consequently incapable of prompting a judgment that the second is definitely nearer, or is definitely farther than the first.

Each series was continued until the observer had reached a considerable degree of confidence in his judgment of nearer or farther. For the sake of convenience, symbols were employed to denote five increasing degrees of confidence. Confidence 1 denoted the minimal degree; confidence 5 denoted absolute certainty. Three approximately equal stages intermediate between these extremes were differentiated, and designated by the symbols 2, 3 and 4.

About ten determinations of each limen were made. In order that time might be saved, the series was seldom continued after confidence 3 had been reached.

(*B.*) Binocular Abrupt Series.

For this series a binocular observation-tube was fitted to the apparatus in place of the monocular tube. The observer now used both eyes in fixating the screen edge. Otherwise, the apparatus and conduct of the experiment remained unchanged.

(*C.*) Monocular Gradual Series.

A modified form of apparatus was employed in these experiments. The frame EFGH was removed; and a smooth hardwood track was made fast to the top of the table at the place indicated by the dotted line T in the diagram. Upon this track ran a carriage provided with grooved wheels. One of the screens used in the previous experiments was fastened to the carriage. About 75 cm. above the surface of the table was fitted a second wooden track parallel with the first; the screen carried on its upper edge a wheel which ran on this upper track. This device made it possible to move the screen to and fro without noise and without tremor. A metal pointer, fixed to the carriage, just cleared a millimeter scale, and indicated the distance of the screen from the observer's eye. The method of conducting the experiment was as follows. The shutter was opened, and the observer was asked to fixate the edge of the screen. When he had obtained a perfect image of the edge, he gave a signal; the screen was now pushed with a constant and uniform motion along the track. The rate of motion employed for all points of the scale was seven centimeters in ten seconds. The observer gave a signal when he first detected movement, and a second signal when he was able to detect the direction of movement.

TABLE I.

Results of Monocular Abrupt Experiments. a. Confidence 1. Showing the points at which Approach and Recession of the Second Screen were first noticed.

		286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
		Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
Settings of Second Screen.	*G.*	270±2	320±13	318±1.6	384±9	375±4	458±7	474±5	563±8	618±17	750±19
	M.	260±3	318±8	305±6	373±6	357±5	458±15	450±7	566±14	593±13	760±21
	S.	258±6	325±9	305±5	376±14	360±8	456±15	446±10	565±10	596±15	747±24
	D.	255±5	334±11	288±6	403±16	352±6	506±26	444±13	604±21	578±16	798±34
Limens[1] in % of Standard Distance.	*G.*	5.6%	12%	4.5%	15.3%	6.3%	14.5%	5.2%	12.6%	7.4%	12.5%
	M.	9.1	11.3	8.4	13	10.6	14.5	10	13.2	11.1	14
	S.	9.7	13.7	8.4	12.8	10	14	10.8	13	10.7	12
	D.	10.9	16.8	13.5	21	12	26.5	11.2	21	13.4	19.7

[1] By 'limens,' as explained above, we here mean the difference in distance between the standard and variable screens at which the observer first passed judgment of 'nearer' or 'farther' with a given degree of confidence.

TABLE II.

Results of the Monocular Abrupt Series. b. Confidence 3. Showing the point at which the Approach or Recession of the Second Screen was observed with a fair degree of confidence.

	286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
G.	252±7	365±12	302.6±8.3	430±32	355±16	512±26	446±17	625±31	602±12.7	847±21
M.	237±7.6	342±6	288±8	399±11	330±6	492±18.4	403±9	607.5±22	550±25	840±45
S.	247±6.6	340±10	287±4.3	402±7.4	340±8	487±11	424±8	610±32	569±18	787±49
D.	230±6.6	376±8.8	278±10	442±35	332±8.3	554±12	432±25	688±42	532±41	880±35
G.	11.9%	27.7%	9.1%	29.1%	11.3%	28%	10.8%	25%	9.8%	27%
M.	17.2	19.6	13.5	19.8	17.5	23	19.4	21.5	17.6	26
S.	13.7	18.9	13.8	20.7	15	21.8	15.2	22	14.7	19.1
D.	19.6	31.5	16.5	32.7	17	38.5	17.4	37.6	20.3	32

TABLE III.

Results of the Binocular Abrupt Series. Confidence 1. Showing the points at which Approach or Recession of the Second Screen was just noticed.

	286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
G.	282±1.5	291.6±1	330±0	337±.8	395±0	405.7±.9	492.5±2.5	508.3±2.2	655±2.5	676.6±2.3
M.	283±0	290±2	327±1.3	340±1.5	393±1	405±0	495±1.7	507.5±2.3	656±3	679±2.8
S.	281±1.3	291±1	327±0	339±1.8	395±1.3	407±2	490±0	507±1.8	650±1.7	673±2.7
D.	277±1.5	293±2	325±.9	342.5±2.5	389±2.4	409±1.5	487.5±3	510±0	647±4	683±3
B.	280±2.3	293±1.7	328.3±2.3	338±2	392.3±1.5	410±0	493±1.3	510±0	653±2.2	680±3.3
G.	1.4%[1]	1.96%	.9%	1.2%	1.3%	1.4%	1.5%	1.7%	1.8%	1.4%
M.	1.1	1.4	1.8	2.1	1.8	1.3	1	1.5	1.7	1.8
S.	1.8	1.8	1.8	1.8	1.3	1.8	2	1.4	1.7	1.8
D.	3.1	2.5	2.4	2.9	2.8	2.3	2.5	2	3	2.4
B.	2.1	2.5	1.5	1.5	1.9	2.5	1.4	2	2.1	1.9

[1] The comparative irregularity of these binocular limens is doubtless due to the relatively small number of binocular determinations.

We give below, in separate Tables, the average limens of movement, and of direction.

TABLE IV.

Monocular Gradual Movement. Showing the points at which movement was first noticed in Approach and in Recession.

	286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
G.	241	375	287	425	344	493	417	601	578	778
M.	220	365	271	420	320	515	410	622	550	807
S.	232	388	280	479	325	570	396	664	550	850
D.	225	380	289	443	328	533	415	620	552	805
B.	240	421	278	507	325	610	405	706	555	927
G.	15.8%	31.2%	13.8%	27.6%	14%	23%	16.6%	20%	13.4%	16.7%
M.	23	27.7	18.6	26	20	29	18	24	17.6	21
S.	18.9	35.7	16	44	19	43	21	33	17.6	27.5
D.	21	33	13	31	18	33	17	24	17.3	21
B.	16	47	16.5	52	19	52	19	41	16.8	39

TABLE V.

Results of Monocular Gradual Series. Direction discerned. Confidence 1. Showing points at which direction of gradual movement was first observed in Approach and in Recession.

	286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
G.	229	415	272	462	323	515	388	620	520	827
M.	212	398	247	469	285	543	366	656	496	848
S.	219	407	251	516	313	596	374	679	511	873
D.	201	513	243	539	284	684	343	770	500	892
B.	183	875	196	683	197	750	234	883	242	—
G.	20%	45%	18%	39%	19%	29%	22%	24%	22%	24%
M.	26	39	26	41	29	36	27	31	26	27
S.	23	42	25	55	22	49	25	36	23	31
D.	30	79	27	61	29	71	31	54	25	34
B.	36	206	41	105	51	88	53	77	64	—

(*D.*) Monocular and (*E.*) Binocular Estimation of Absolute Distance.

The form of apparatus described under (C) was employed in the experiments upon monocular and binocular estimation of absolute distance. The screen L_1 was set at any one of the distances to be estimated (286 mm. up to 667 mm., and 900 mm.); the observer fixated the edge, and estimated its distance in inches.

Table VI.

Estimates of Absolute Distance—Monocular.

	286 mm.	333 mm.	400 mm.	500 mm.	667 mm.	900 mm.
G.	185 ± 33	236 ± 41	342 ± 52	386 ± 33	546 ± 68	603 ± 25
M.	183 ± 28	249 ± 43	354 ± 42	412 ± 46	569 ± 43	656 ± 41
S.	188 ± 46	285 ± 66	345 ± 38	452 ± 25	610 ± 33	823 ± 39
D.	163 ± 31	189 ± 34	234 ± 28	343 ± 28	439 ± 51	585 ± 43
B.	407 ± 72	367 ± 39	325 ± 46	372 ± 76	467 ± 82	435 ± 37

Relation of Estimated to Actual Distance—Expressed in Per Cents.

G.	65%	71%	86%	77%	82%	67%
M.	64	74	89	82	85	73
S.	66	86	86	90	92	92
D.	57	56	59	69	66	65
B.	142	110	81	74	70	48

III. Discussion of Results.

(*A.*) Monocular Abrupt Series.

Several features stand out prominently in the results of these experiments. 1. The differential limens of approach bear an approximately constant relation to the stimulus distances. 2. The limens of recession are also fairly regular throughout the series. 3. The limens of approach are uniformly less than those of recession. 4. In all cases the limens are less than one would expect from Hillebrand's investigation with the same apparatus. 5. The capacity to sense differences of distance shows a well-marked individual variation. 6. The smallness of the mean

variation indicates that all four observers estimated changes of distance with a high degree of definiteness.

TABLE VII.

*Estimates of Absolute Distance—Binocular.**

	286 mm.	333 mm.	400 mm.	500 mm.	667 mm.	900 mm.
G.	198 ± 28	279 ± 25	366 ± 41	432 ± 35	513 ± 51	653 ± 48
S.	185 ± 15	233 ± 33	300 ± 38	422 ± 32	630 ± 59	884 ± 48
D.	252 ± 23	300 ± 25	348 ± 11	429 ± 51	493 ± 36	744 ± 28
B.	254 ± 11	310 ± 18	346 ± 33	422 ± 77	539 ± 58	713 ± 84

Estimated Distance in per cent. of Actual Distance.

G.	69%	84%	92%	86%	77%	73%
S.	65	70	75	84	95	98
D.	88	90	87	86	74	83
B.	89	93	87	84	81	79

*Through an oversight, *M's* series of binocular estimates was left incomplete. Hence his name does not appear in this Table.

None of these features is peculiar to our investigation alone. Wundt's original thread experiments[1] give at least a hint of the regular increase of limen with increase of distance; while in Arrer's results[2] the regularity of increase is but little less pronounced than in our own. Wundt's early experiments brought to light the fact that approach is more readily perceived than recession; this observation was confirmed in the main by Arrer's results, though exceptions were found also by Hillebrand and Dixon. Hillebrand's investigation points to an unusually large differential limen;[3] indeed, a difference of half a diopter seems to be the smallest limen determined for any of his observers. Yet Dixon, under identical experimental conditions, found 77% of right judgments when the screens were separated by only .05 diopter difference.[4] In Wundt's experiments the limens varied between 4% and 11% of the standard distance; Arrer, also employing a thread for fixation-

[1] *Beiträge*, p. 114.
[2] *Phil. Studien*, XIII, pp. 139-142.
[3] *Zeitschrift*, VII, pp. 126-9.
[4] *Mind*, N. S., IV, p. 210.

object, obtained limens as low as 1.6%. The maximal and minimal variations determined by Arrer[1] indicate that the mean variation of his observers was even less than that of ours. Individual variations are common to the results of Hillebrand, Dixon and Arrer.

The judgments of one of our observers—*B*—are omitted from our tabulated results. It was discovered early in the course of the experimentation that *B* differed essentially from the other observers, not only in degree of readiness and correctness of judgment, but also, to all appearances, in the nature both of the criterion and the conscious process involved. *B*, *G*, *M*, and *S* almost invariably gave their judgments an instant after the appearance of the second screen; it was found that if they hesitated, even for only a few seconds, they were lost. They seldom made an error; almost never did they give a wrong judgment with any considerable degree of confidence. In 86 determinations *G* had only 5 wrong series of judgments which reached Confidence 3; *D* in a total of 83 had 4 wrong; *M* and *S* had none wrong in totals of 93 and 88 respectively. *B's* judgments never came instantaneously; after the exposure of the second screen, there always elapsed a period of hesitation during which *B* was evidently deliberating and groping about for a clue. His judgments were usually given in such terms as: "It must be farther" (referring of course to the second screen); "It is less clear-cut; I should say nearer;" "It is more distinct; I think it is farther." These judgments were never characterized by the immediacy which seemed to be a necessary condition in the case of the other observers. Attempts were frequently made to draw a judgment from *B* soon after the second screen appeared, but always without success. If the second screen came into view with its edge blurred, the indistinctness persisted for some seconds, and frequently refused to disappear even after a tedious delay. It was found to be impossible to get the full complement of determinations from *B* although he gave more sittings than any other observer. Twenty-five single judgments in the hour was a limit beyond which he could not go. He usually suffered from fatigue in the eye, towards the close of the sitting; *D*, *G*, *M* and *S* averaged 80 or 90 judgments in the hour. While they rarely made an erroneous judgment, *B* seldom carried a set through without error. Of his 53 series, 26 were absolutely wrong, even when Confidence 3 was reached.

There can be no doubt that *B's* criterion was wholly different from that employed by the other observers. The immediacy of their judgments is in striking contrast with his delib-

[1] *L. c.*, p. 142.

eration; his inaccuracy is the more surprising in view of their precision. Since his accommodation failed to change on the appearance of the second screen, or changed so slowly and uncertainly as to be attended by no definite sensible effects, he was compelled to have recourse to another criterion. He seems to have hit upon a clue in dispersion-images, and to have inferred nearness from indistinctness. If, on the other hand, the edges seemed approximately equally clear-cut, the second screen was usually judged farther, even when the two were equi-distant. He frequently reported that the screens were unequally dark, and judged the darker screen to be nearer. It is extremely doubtful if there was any difference in the brightness of the two screens. Care had been taken to make them as nearly identical in appearance as possible; no other observer discovered any inequality of illumination, and *B*'s introspections in the binocular experiments make no mention of unequal brightness.

Here are the records of a few typical series:

Obs. *B*. Feb. 22, 1902. 10 A. M. Condition normal.

Distance of Standard Screen.	Distance of Variable Screen.	Judgment.	Confidence.
667 mm. (left screen)	667 mm. (right screen)	Second screen darker. Seems nearer.	2
667 (right)	667 (left)	Second lighter. Must be farther.	1
667 (left)	667 (right)	Second lighter. Seems farther.	2
667 (right)	667 (left)	Equidistant.	

Obs. *B*. Jan. 24. 10 A. M. Condition normal.

500—500	Equal.			
" —up to 590	Equal.			
" —600	Second less distinct.	Nearer.		2.
" —608	" " "	"		2.
" —616	Second less distinct but lighter.		Farther.	1.
" —624	Second less distinct.	Nearer.		2.
" —632	Second less distinct but lighter.		Farther.	1.
" —640	" " " "	"	Farther.	2.
" —648	Second less distinct.	Nearer.		2.
" —656	" " "	and darker.	Nearer.	2.
" —664	" " "	" "	"	3.

Obs. *B*. March 8. 11. A. M. Normal.

333—333	Equal.		
333—up to 375	"		
333—380	Second nearer.	Cannot say why.	1.
333—390	" "	Darker.	2.
338—400	" "	less distinct.	2.
333—410	" "	" "	2.
333—420	" "	darker.	2.
333—430	" "	less distinct.	2.
333—440	" "	" "	3.

Obs. *B.* March 15.	10 A. M.	Normal.
286—286	Equal.	
289—up to 410	Equal.	
286—415	Second farther—less effort to see it.	2.
286—up to 490	" " " " "	2.
286—495	" nearer. Edge is blurred.	2.
286—500	" " " " "	2.
286—505	" " " " "	1.
286—510	Equal.	
286—515	"	
286—520	Second farther. More easily focused.	2.
286—525	" " " " "	3.

During the progress of this latter series *B* remarked that he hoped for change in the direction of nearer. He fears "farther" experiments, because he does not understand how he could judge in them.

No such series of judgments was given by any other observer. The following are typical of the records given by *D*, *G*, *M* and *S*:

Obs. *M.* Jan. 2, 1902.	10 A. M.	Normal.
400—400	Equal.	
400—442	Second nearer. Cannot say why.	1.
400—449	" farther. Cannot say why.	1.
400—456	" " "flashed up farther."	1.
400—463	" " " " "	2.
400—470	" " " " "	2.
400—477	" " " " "	2.
400—484	" " " " "	3.

Obs. *G.* Feb. 20.	11 A. M.	Normal.
500—500	Equal.	
500—492	"	
500—484	Nearer. "Simply came in nearer."	1.
500—476	" "Came in nearer."	2.
500—468	" Slight feeling of strain with second screen.	2.
500—460	" Slight feeling of strain with second screen.	2.
500—452	" Feeling of strain more pronounced.	3.

Obs. *G.* March 27.	11 A. M.	Normal.
286—286	Equal.	
286—315	Equal.	
286—320	Possibly nearer. Cannot say why.	1.
286—325	Farther. " " "	1.
286—330	" " " "	1.
286—335	" Less strain with second screen.	2.
286—340	" " " " " "	2.
286—345	" " " " " "	2.
286—350	" " " " " "	2.
296—355	" " " " " "	3.

G reported a sensation of eye-strain in nearly every series. He found the sensation to be more intensive when the second screen was nearer, less intensive when it was farther. This

sensation seemed to furnish the basis for his judgments throughout the whole investigation. Only in rare cases did *D*, *M* and *S* report a sensation of strain. They usually failed to find any basis for their estimation. "It just seems nearer (farther); that is all." *G's* judgments, however, were not a whit less immediate than those of *D*, *M* and *S*. It was only when he came to introspect, that he mentioned the sensation of strain. Care had been taken to avoid the suggestion of this sensation to any observer. Indeed, when we seemed to discredit the presence of *G's* sensation of strain, he insisted the more strongly that he could not be mistaken in it. It is worth noting that the only observer who was able to discriminate between his positions of accommodation by a distinct consciousness of strain, has much lower limens of approach than the others. (See pp. 178 ff.)

Obs. *S.* Feb. 15.	3 P. M.	Normal.
500—500	Equal.	
500—up to 558	"	
500—564	Slight difference. Second perhaps farther.	1.
500—572	Second came in farther.	1.
500—580	" " " "	2.
500—588	" " " "	2.
500—596	" " " "	2.
500—605	" " " "	3.

Obs. *D.* March 13.	2 P. M.	Normal.
333—333	Equal. "Both horribly near."	
333—up to 400	Equal. "Very unpleasant."	
333—405	Second seems farther.	1.
333—410	" " "	1.
333—415	Farther. Simply comes in at greater distance.	2.
333—420	" " " " " "	2.
333—425	" " " " " "	2.
333—430	" " " " " "	2.
333—435	" " " " " "	3.

These specimen records show the general character of the two types of judgments and introspection. The one type is characterized by its immediacy; the most searching scrutiny failed—save in the case of *G* alone—to find any introspective evidence of the presence in consciousness of a sensation-basis for the judgment. *G's* estimations were no less direct in character, but introspection almost invariably revealed the presence of a sensation factor which made the judgment possible. The other type of judgment is anything but immediate in character. Not only was the time interval between the exposure and the judgment invariably longer, but introspection discovered conscious processes, not muscular but retinal, by which the judgment was finally mediated. Moreover, as has already been pointed out, the types are as divergent in accuracy as they are in character. We believe that we are justified in assuming

that the judgments of persons possessed of the normal power of accommodation, belong to the former type.

An attempt was made, towards the close of the series of monocular abrupt experiments, to determine how far a 'conscious impulse of will,' as assumed by Hillebrand, could play a part in the judgments of relative distance. The observers were directed to make a voluntary change of accommodation from near to far or from far to near, when the second screen appeared, and to estimate its relative distance in the light of what transpired. The results invariably tell against Hillebrand's assumption. It was usually discovered that the observers were unable to change their accommodation at will in the sense assumed by Hillebrand. It was found, however, that they could see the second screen distinctly by simply willing to see it distinctly. But *such an act of will furnished no conscious datum of central origin which could give a clue as to the direction in which the change of accommodation had been made.* In some few cases the appropriate change of accommodation was brought about voluntarily, but this procedure proved to be a hindrance rather than a help to the accurate estimation of relative distance. In short, the unanimous verdict of all four normal observers goes to show that if the judgment is not made immediately it cannot be made at all.

Whether the cultivation of such an artificial means of estimating distances as Hillebrand describes could yield good results, even in the hands of observers trained to a voluntary control of their accommodation mechanism, is exceedingly problematical. Except that he gives it a name, Hillebrand leaves us absolutely in the dark as to the basis of the judgment. From what source does the conscious datum come which serves to determine what our judgment in a given case shall be? Whence do we get the raw material which enables us to differentiate, to judge now nearer, now farther? If, with Hillebrand, we deny that peripheral sensations are aroused in the adjustment of the ocular mechanism, we find ourselves compelled either to deny the possibility of estimating space monocularly, or to resurrect the deceased theory of innervation. To choose the former alternative means to dispute the results of Wundt, Dixon, Arrer and Hillebrand himself. To choose the latter, means to maintain that the voluntary initiation of movement is attended by a sensation of central origin, whose intensity is gauged by the amount of energy put forth into the movement. This in turn means to ignore the results of several decades of physiological and psychological research, and to take one's stand upon a theory long since discarded.

The results of the twenty-seven correct determinations made

by *B* are appended—not for comparison, because they are for obvious reasons incomparable with the results of the other observers, but solely in the interests of completeness.

TABLE VIII.

Results of Monocular Abrupt Experiments. a. Confidence 1. Showing averages of B's Correct Determinations.

286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
250±4	420±12	297±5	407±39	366±9	500±17	473±4	640±13	610±12	775±25
12.6%	47%	10.8%	22%	8.5%	25%	5.4%	35%	8.6%	16.2%
b. Confidence 3.									
235±3	560±0	285±15	457±29	323±18	520±26	450±20	720±31	525±45	865±0
17.9%	96%	14.4%	37.2%	19.3%	30%	10%	44%	21.3%	29.7%

(*B.*) Binocular Abrupt Series.

The principal features of these results are: 1. The liminal values are extremely small. 2. There is a large individual variation among the observers. 3. The recession limens are in general greater than those of approach—although in about 40% of the determinations they are approximately equal. 4. The proportion of limen to stimulus is approximately constant both in approach and in recession. 5. The most striking feature of Table III is, however, in connection with *B's* results. *B's* monocular judgments of relative distance were a failure; his binocular judgments compare favorably with those of the other observers. 6. It is to be noted, too, that *G's* monocular limens—particularly those of approach—were much smaller than those of any other observer; whereas his binocular results do not differ materially from the average determinations.

The introspections of this series show the presence here of but a single type of judgment—and that of the immediate character already described. *B's* introspections differ in no essential particular from those of the other observers. He reports that "the second screen came in at a greater distance," or that "it simply stands out in front," etc. In comparatively few instances did an observer succeed in discovering the factor on which he based his judgment; it was described as a sensation of strain in the two eyes—and it occurred chiefly with near distances. The localization of this sensation was indefinite; no observer was able to state that its origin was in the external muscles of the eyeball and not in the ciliary muscle.

As in the monocular experiments, so here it was found that the judgment must be given without delay. If hesitation arises, doubt ensues and estimation becomes impossible. An introspection of *B's* reads as follows (screens at 500 mm. and 492 mm.): "The second screen stands out nearer to me. No difference in clearness; no noticeable difference in strain," and at another time: "the second simply came in nearer. After a moment I could no longer be sure that they had been different." Apparent recession on continued fixation of a near screen was remarked by all observers.

(*C.*) Monocular Gradual Experiments.

These results show that: 1. Movement was almost invariably perceived before direction was discerned. 2. Limens both of approach and of recession are higher with gradual than with abrupt change of distance. 3. Limens of approach are uniformly lower than those of recession when the change of distance is gradual. 4. *B's* limens are in every case higher than those of the other observers; indeed *B* detected no recession be-

tween the limits of 667 mm. and 950 mm. (the extreme limit of the apparatus); but 5. *B's* errors are comparatively insignificant in the gradual experiments. In a total of 90 series he had but 9 errors, 8 of which were failures to detect recession of the screen.

The introspections in this series bring to light the same two types of judgment as were found in the monocular abrupt experiments. Here, as formerly, *D*, *M* and *S* gave immediate judgments for which no evidence could be found in consciousness. *M* failed in all cases to describe the conscious process which led to his judgment. *S* reported, over and over again: "I felt it go away (come nearer). No blur; no strain." *D* was frequently startled in the approach experiments by the suddenness with which "it seems to rush up toward me." *G* usually reported perceptible increase or decrease of strain. On the other hand, *B* seemed in all cases to infer the direction of motion from the rate of blurring of the screen-edge; if the blurring increased rapidly, he inferred approach; if slowly, recession.

It was suspected that the other observers were unconsciously employing the same criterion. But the results obtained from a modification of the experiment showed that this suspicion was groundless. Series were occasionally introduced, in which the screen was made to approach with extreme slowness, and to recede with corresponding rapidity. This change of method was followed by no change in the accuracy of the judgments of *D*, *G*, *M* and *S*, but it invariably led to an error in *B's* estimation. It is safe to conclude that all of *B's* experiments with gradual change of distance are failures, so far as the muscular sensations of accommodation are concerned.

Hillebrand's observers were unable to estimate gradual changes of distance with any degree of accuracy. They usually refused even to offer a conjecture as to the direction of the movement; and when they did make a judgment, they were as often wrong as right. It is to be noted that Hillebrand's results are really incomparable with ours on account of the difference of experimental conditions. Hillebrand's movement began before the shutter was opened, and hence may be assumed to have been well under way before the observer secured his first accommodation upon it. We repeated a few of his experiments; and while we did, in some few cases, obtain positive results, still the judgments were vague and indefinite and frequently erroneous. His method was finally abandoned, for two reasons. 1. If, when, the fixation-object first comes into view, it is at once near to the observer's eye, and moving directly towards or from him, accommodation upon it must necessarily be difficult and uncertain. Hence the negative

character of the results of experiments with such a fixation-object does not justify the conclusion that accommodation *never* contributes to the perception of distance. It can only be said that experiments of this type are from their very nature inadequate to a positive solution of the problem in hand. 2. The method would be inaccurate and indefensible even if it yielded results of a positive character. For if the screen be in motion before the observer accommodates upon it, we have no means of determining its precise position at the instant when he first obtains a clear image. Hence when the judgment of 'nearer' or 'farther' is finally given, we still have no record of the initial position to which it refers. It is obviously impossible to determine the relation of two positions of accommodation, one of which is, and must remain, unknown. Hence the results of such experiments cannot contribute to a solution —positive or negative—of the problem.

It would be interesting to learn how Hillebrand would reconcile the negative results of these experiments with his assumption of a 'conscious impulse of will,' by which changes of accommodation are accomplished, and mediated to consciousness. Changes of accommodation must have come about, if the moving screen were seen in perfect definition when it stood at different distances. If, now, these changes were the product of a '*conscious* impulse of will,' how did it happen that the observer was unconscious of change of distance?

Hillebrand's experiments with the Aubert diaphragm are also said by their author to tell against the participation of sensations of accommodation in the perception of depth. He found that when the diaphragm was slowly advanced, its aperture meanwhile being rapidly decreased, there resulted a judgment of recession, notwithstanding the greater tension of accommodation. Here, again, his demonstration is as fatal to his own theory as it is to that of Wundt.

This experiment, however, is irrelevant to the question: we are not concerned with the determination of the relative significance of the various criteria of distance. It fails to disprove that sensations of accommodation may contribute to the perception of depth. That these sensations constitute the sole—or even the most effective—factor of depth-localization has never been maintained. Decrease of visual angle was obviously the determining factor in this experiment. It is not incomprehensible that a factor which is effective under favorable circumstances may be swamped and rendered inoperative under adverse conditions. From the point of view of accommodation, the change of visual angle was a distracting element of superior power. That the stars cannot be seen at noon is no proof that they are forever invisible.

D and *E*. THE ESTIMATION OF ABSOLUTE DISTANCE.

The method of recording the estimates in our experiments was not free from objection. The estimate was expressed in terms of inches; it resulted from the comparison of the perceived stimulus distance with a distance mentally reproduced. For this reason, one mnst be cautious in drawing conclusions from our tabulated results, which make a direct comparison of the recorded estimates with their objective stimuli.

Whether or not these results may be accepted as a corroboration of the principle that distance is ordinarily under-estimated in the absence of secondary criteria, they at least establish the fact that absolute distance was estimated with some degree of definiteness by our observers.

It has been asserted by Wundt[1] and repeated by Hering[2] that under conditions such as were present in our experiments, the absolute distance of the fixation-object cannot be estimated. This statement is not in accord with our results. Even at their first sitting, our observers were able to express their judgments in absolute terms. In the initial experiments they were asked to estimate, not the relative position of the second screen, but the absolute distances of both screens. This they seldom failed to do; page after page of our earlier records of the monocular abrupt experiments is filled with such results as these:

Stimulus distances.	*Estimates. (Recorded in inches but here expressed in mm.)*
333 mm.—500 mm.	229 mm. and 305 mm.
667 " —333 "	457 " " 254 "
500 " —250 "	305 " " 127 "
667 " —667 "	381 " " 381 "
333 " —250 "	254 " " 178 "
333 " —310 "	254 " " 254 minus mm.

The possibility of absolute localization cannot be denied. Our results can teach us little, however, of the capacity to localize definitely (with small mean variation) and correctly (in agreement with reality).

IV. INTERPRETATION AND CONCLUSIONS.

The object of this investigation is to determine the influence of accommodation and convergence upon the perception of depth. Our experimental results have already pointed to a positive solution of the problem.

The investigation has shown that under experimental condi-

[1] *Beiträge*, p. 107.
[2] *Raumsinn*, pp. 546-7.

tions which exclude all known criteria of depth—save only accommodation and convergence—it is still possible to perceive changes of distance. This establishes the fact that either accommodation, or convergence, or both contribute to the perception of depth.

It has shown, too, that the absence from monocular vision of normal sensitivity to relative distance is not necessarily attended by a corresponding abnormality in binocular vision. This demonstration warrants the conclusion that the factors which determine the judgment of relative distance are essentially different in monocular and in binocular vision. This conclusion is supported by the fact that the limens of approach and recession bear an essentially different relation to the stimulus-distances in the two cases.

It has also been shown that in monocular vision—when the factors present are reduced to accommodation and convergence—the presence of normal accommodation has been attended by the capacity accurately to estimate relative distances, while the absence of normal accommodation has been paralleled by an absence of this capacity. From this we conclude that *accommodation constitutes the essential criterion of depth in our monocular experiments.*

It follows from our binocular results that the essential criterion in the binocular experiments was not furnished by accommodation.

The hypothesis that increased tension of accommodation gives rise to judgments of approach is not new in the literature. This theory was advanced by Berkeley, and has never since his time been without supporters. That the judgment of recession can be prompted by the opposite change of accommodation has never been generally maintained. Indeed Wundt explicitly asserted in his original paper[1] that this position is untenable. He held that since the change of accommodation which focuses a receding object at successive distances is accommplished by the relaxation of the ciliary muscle, it cannot give rise to sensation, and hence cannot constitute the basis of a judgment. This position is the logical consequence of the theory of innervation-feelings which Wundt then advocated.

If however it be held, as is now all but universally believed, that we become conscious of change of muscular tension, not through the medium of feelings of innervation, but through concomitant peripheral sensations, there is no ground for denying that the relaxation of muscle is attended by sensible effects.

[1] *Beiträge*, p. 110.

Muscle and tendon are richly supplied with sense-organs. It is conceded that the different terms of an ascending series of muscular contractions are discriminated. It is but reasonable, then, to suppose that the individual terms may still be differentiated, when the series recurs in descending order, from tension to relaxation,—*i. e.*, from greater to less degree of contraction. Hence, we are justified in concluding that accommodation may prompt judgments alike of farther and of nearer. This simply assumes that a degree of contraction of the ciliary muscle which is attended by muscular sensation if it follow upon a lesser degree, will be similarly attended if it follow upon a greater degree of contraction.

Histological investigation has shown that the organs of the muscular sensations are anatomically akin to the cutaneous organs of pressure. Von Frey's experiments[1] demonstrate—what, indeed, cannot well be disputed—that we are able to discriminate the successive stages not only of an ascending series of cutaneous pressure, but of a descending series as well. Moreover, von Frey is convinced that the differential limen is higher in the latter case than in the former.

If now this conception be carried over from the pressure-organs of the skin to the similar organs of muscle and tendon, one would expect to find that changes of accommodation arouse muscular sensations, whether they be accomplished by muscular 'contraction' (ascending series) or by muscular 'relaxation' (descending series of contractions). These two states of muscle are not different in kind; the latter is itself formulable in terms of contraction.

Since von Frey found that the differential limen of increase of serial pressure on the skin is lower than that of decrease, one would again expect to find that the same relation obtains between the differential limens of muscular 'contraction' and muscular 'relaxation.' In fact, the results of our monocular experiments prove that the limen of approach is invariably less than that of recession.

An attempt was made to photograph the images reflected by means of a phacoscope from the anterior and posterior surfaces of the lens when it was accommodated for each of the five standard distances and of the distances given in Table I as just noticeably nearer and farther. It was hoped by this means to determine the radii of curvature of the two surfaces of the lens in each of the fifteen positions of accommodation. From these data, assuming the volume of the lens to remain constant, the determination of the magnitude of its change of diameter for these various positions of accommodation seemed possible. The change of diameter would then indicate the liminal lengthening and shortening of the ciliary muscle. Several difficulties were encountered, however. It was found to be impossible, with any

[1] M. v. Frey: *Untersuchungen über die Sinnes-functionen der menschlichen Haut. Erste Abhandlung*, pp. 180-188.

available light intensity, to obtain a photograph of the lenticular images. The table of radii of curvature published by Weiss[1] furnished, with interpolations, the data for the anterior surface. Similar data for the posterior surface could not be obtained; though the changes of the latter surface are relatively slight, they can scarcely be ignored. Doubt as to the relative parts played by the straight and annular fibres of the ciliary muscle seemed likely to vitiate our results. Moreover, uncertainty as to the area of the lenticular surface affected by the change of curvature made accurate calculation impossible, and the plan was finally abandoned.

The essential features of our conclusions regarding the perception of relative distance in the monocular experiments may be summarized as follows:

(1) Both the approach and the recession of the fixation-object can be perceived.

(2) These changes of distance are perceived from the corresponding changes of accommodation.

(3) Muscular sensations of accommodation constitute the sense-basis of the perception both of approach and recession.

V. Theory.

When we attempt to bring our results into relation with one or other of the modern theories of psychological space, we must, of course, recognize that these theories are much broader in scope than any single investigation can be. A theory of space must pronounce upon many questions which our problem leaves untouched. Yet it may with justice be maintained that the results of an experimental enquiry—however circumscribed its province—cannot but contribute to our knowledge of the general topic of which it forms a part. Hence it is probable that the conclusions reached in the present study may aid us in adjudicating the relative merits of the rival theories of space,—particularly since it has been concerned with the question upon which the rivals are most at variance.

It is in dealing with the phenomena of relative localization in binocular vision that Nativism has been most successful. The presence of a pair of retinas furnishes a natural basis for the assumption of a paired arrangement of retinal points. From this naturally follows the assumption that retinal disparity and double-images constitute the criteria of relative depth. The theory so far is plausible and alluring; its simplicity of conception and clear envisagement of the process of localization are strong points in its favor. Yet the ultimate criterion of the worth of any scientific theory must be its ability to account for all the facts which it seeks to systematize. And it cannot

[1] O. Weiss: *Tabelle der zur Accommodation auf verschiedenen Entfernungen nöthigen Linsenwölbungen. Pflüger's Archiv*, XXVIII, 1902, p. 91.

be denied that however creditably Hering's theory may have acquitted itself as regards the binocular estimation of relative distance, its defects become apparent as soon as it passes beyond this narrow field. Its account of the binocular estimation of absolute distance, and its explanation of the phenomena both of absolute and of relative localization in monocular vision, must be rejected. Moreover, its failure to co-ordinate the phenomena of monocular, with those of binocular vision, casts suspicion upon the whole theory.

The Nativistic theory, whose conception of the process of binocular localization is outlined above, accounts only for the estimation of relative distance. The magnitude of retinal disparity is measured by reference to the retinal center; this central point of reference, is, in terms of the distance of the object imaged upon it, not a fixed but a variable quantity,—since the distance of the point of fixation changes with every change of convergence. Hence it follows that the disparity criterion can furnish only relative depth-values. These values are expressed in terms of the momentary position of convergence, and contribute to a localization relative to the momentary fixation-point. Further than this, a purely retinal theory cannot go. Attempts have been made, however, by Hering and by Hillebrand, to eke out this limitation by a supplementary hypothesis, which we shall later consider.

It is difficult to see how double-images and retinal disparity can furnish an unequivocal criterion of change of distance. Double-images arise, not only when the second fixation-point is nearer, but also when it is farther than the first,—the only difference being the presence of crossed disparity in the former, of uncrossed disparity in the latter case. Hence the assumption that double-images constitute an unequivocal criterion of change of distance, is tantamount to the assumption that crossed disparity can be distinguished from uncrossed, even when their magnitudes have diminished almost to the vanishing point. Experiments with Hering's falling ball apparatus have established a differential limen of about $\frac{1}{50}$. The difference of retinal disparity which is here assumed to be distinguished, approximates to an infinitesimal quantity. Yet Hering's theory asserts not only that these minute differences were detected, but also that there was in every case a consciousness of the kind of disparity present.

Laboratory experiments have frequently shown that observers of normal sensitivity to changes of depth have no immediate knowledge of the kind of disparity operative in a given instance, even when the double-images are manifestly present in considerable degree. The nature of the disparity can be discovered only indirectly, by closure of the one eye and observation of the surviving images with the other. We are not un-

mindful of the familiar phenomena of stereoscopic vision; but we fail to find that Hering's bold statement of the presence of disparity furnishes a satisfactory explanation of the facts in question. The tacit assumption of a retinal capacity, for which experiment gives no warrant, is an insecure foundation upon which to build a theory.

Attention has already been called to the fact that double-images were not consciously present in our binocular experiments. Moreover, it was frequently demonstrated that their intentional production rendered estimation impossible.

The apparent impossibility of distinguishing the double-images occasioned by a farther, from those occasioned by a nearer object, together with the inefficiency of double-images in our own experiments, leads us to reject Hering's criterion as inadequate to the task which it is called upon to perform.

Nor is this the only objection to Hering's conception. The impossibility of finding in the single retina a psychophysical substrate analogous to that posited as ths essential condition of binocular localization, has necessitated the introduction of an extraneous depth-factor. Monocular localization in depth is held to be a function of the 'conscious impulse of will' through which changes of accommodation are made.[1] Monocular and binocular localization are thus differentiated, in that different sense-data and different mental processes are assumed in the two cases. Changes of distance are *seen* in binocular vision; in monocular vision they are in some way discerned through the mediating influence of will.

Here again Hering's assumption is given an unqualified denial by our results. The judgments of our observers followed no less immediately upon the exposure in the monocular than in the binocular experiments. No observer was able to discover a difference in kind between the mental processes involved. We grant, of course, that the levelling effect of time may have modified the processes in question. But that, even after the lapse of ages, two such essentially different processes should have become identical for introspection is scarcely possible. It seems incredible that observers trained in introspection should, in hundreds of trials, invariably fail to detect a difference between a purely sensational process, and a process which is anything but sensational in character.

A further objection to the will criterion assumed by Hering and Hillebrand, is the difficulty of giving it a definite con-

[1] Hering brings forward the principle of coincidence of attention with regard, to account for the localization of the point of fixation; but he identifies the function of attention with that of will (*Raumsinn*, pp. 534 ff.). Hillebrand works out this concept in greater detail and posits a conscious impulse of will as the essential factor (*Zeitschrift*, VII, p. 147).

scious value. Unreasonable as it is to suppose that change of accommodation is ordinarily accomplished by voluntary movement, the matter becomes even more arbitrary when we have to assume that the amount and kind of impulse of will are given us in immediate experience. Just how we become conscious of these minute gradations of will-impulse remains a mystery. Muscular sensations of accommodation would furnish the most natural explanation; but that route is barred, inasmuch as it was the denial of the existence of such sensations that necessitated the impossible hypothesis of conscious impulses of will. Unless we concede the validity of the discarded theory of innervation-feelings, Hering is as far as ever from a satisfactory solution of the problem,—and this concession he can scarcely hope to obtain.

Our examination of Hering's theory has revealed the following defects:[1]

1. It is based upon an assumed retinal capacity which does not exist.
2. Its appeal to the influence of will makes a term do duty for an explanation.
3. It makes an unnatural breach between the phenomena of monocular and of binocular vision.

Our conclusions agree in the main with those of Wundt and Arrer. Upon only a single point—the relative significance of accommodation and convergence in monocular vision—do we take issue with these writers. We have found reason to believe that accommodation is the determining factor in monocular vision, while convergence appears to gain the upper hand in binocular vision. The existence of sensations of accommodation and convergence, is an assumption without which our results cannot be explained. It is true that these sensations received uniform introspective confirmation only in the case of a single observer. But all observers bear occasional witness to their presence. And it is of the essence of Wundt's theory of psychological space as a synthetic function, that the sense-factors involved should, except under the most favorable experimental conditions of analysis, disappear from separate view.

[1] It may perhaps be of interest to learn that the writer began the experimental investigation with a decided leaning toward the theory of Hering. Its conception of the binocular arrangement of retinal points appealed to him as offering a satisfactory psychophysical substrate for the estimation of depth. Hillebrand's apparatus seemed, as it seems even yet, to offer the most advantageous features for an experimental investigation of the problem. Moreover, the criticisms of Wundt and of Arrer seemed, and still seem, to be in several cases unjust. The writer's change of view has been brought about chiefly by the results and introspections of his observers, but also by a more critical examination of the work published by Hering and Hillebrand.

STUDIES IN THE PSYCHOLOGY AND PHYSIOLOGY OF LEARNING.[1]

By Edgar James Swift.

I. On Tossing and Catching Balls.
II. On Learning Short-Hand.
III. On the Acquisition and Control of the Reflex Wink.

Learning, though as various as human activity, may be roughly classed under one or the other of three types: The acquisition of skill, or learning to do; The formation of associations, or the acquisition of information; and the getting of control, or the formation of inhibitions. While it is doubtless true that it would be difficult, if not impossible, to find absolutely pure types of any of these, so fully is mental action of all sorts involved in each, yet for purposes of preliminary study the classification is adequate and each type is considered in the studies that follow.

I. On Tossing and Catching Balls.

The purpose of this study was to investigate the learning process, to inquire whether there is a typical curve of the acquisition of skill, and if so to determine its general form and so far as possible to find out what alters it in particular cases.

Method, apparatus and subjects. Keeping two balls going with one hand, receiving and throwing one while the other is in the air, was selected. There were several reasons for deciding upon this, some of which can be best appreciated by those who have tried to do it, but the chief argument in its favor was the accuracy that it permits in measuring the learner's progress.

The balls used were of solid rubber and weighed 122 6/10 and 130 2/10 grammes. This difference was not noticed by the subjects. Their diameters were 42 and 44 millimetres, respectively. Six subjects in all were tested; five with the regular series and one in keeping three balls up with two

[1]The writer wishes to express his obligation to Dr. Edmund C. Sanford for his generous co-operation throughout this investigation. The President and other members of the faculty of Clark University have also shown interest, and the assistance of the librarian, Mr. Louis N. Wilson, in securing literature has been invaluable.

hands. Five of the subjects were university students and one was a professor.

The daily programme consisted of ten trials, the subject in each case continuing the throwing until he failed to catch one or both of the balls. The number of catches made in each trial was immediately recorded with any data obtainable as to the method pursued and the cause of failure. After each trial the subject rested as long as seemed necessary and then recommenced the throwing until the ten trials had been completed. There was no practice whatever between the tests and none of the subjects had ever handled balls in this way except as the base ball and tennis player may occasionally throw a ball or two into the air and catch them as they come down. All the subjects did the work in the afternoon. In the few instances in which a change of hour was necessary this fact was recorded. The total time occupied in the testing (and this testing constituted also the sole training of the subject) was various in amount extending from a few minutes in the early stages of practice to two or three hours toward the end. All the subjects knew their daily score and they always kept track of their progress during each test as well as from day to day. This method has undoubtedly given results different from those that would have been gotten had the subjects been kept in ignorance of the score, but the plan was uniform throughout and had the advantage of largely overcoming the effect of monotony which usually depresses those who are obliged to practice continuously for so many days. Besides, it enabled observation to be made, incidentally, on the effect of competition both with one's own record and with that of others.

The throwing and catching was chiefly with the right hand in each case (all the subjects were right-handed) but in order to test the effect of right-handed practice upon the skill of the left hand a preliminary test was made upon each subject, on the first day of his service, of his untutored skill with the left hand. This preliminary test consisted of ten trials as usual; and after it the left hand was not again tried until after the completion of the whole period of work with the right hand, when the left hand was again tested and a record of its progress kept for a number of days.

The daily training was continued in the case of four of the six subjects until their average number of catches per trial exceeded 100, or, what amounts to the same things, 1,000 catches in ten trials, upon two days in succession. In the case of the other two subjects the training was broken off at a lower score for reasons that will appear later.[1] The tests with the right

[1] That even the lesser skill attained by these two was not bad seems evident when we find Hopkins saying that "the young man who can

hand were made every day, including Sundays, as were also those with the left, except as indicated below.

Still another phase of the experiment was an attempt to find how skill in the management of the balls declined after the cessation of daily practice. With this in view monthly tests have been made since the close of the regular experiments with the right hand, upon three of the subjects. The results of these "forgetting" tests will be given in their proper order below.

Influences that affected the score. It seems probable that the weight of the balls may have had an influence on the results on account of fatigue, and tennis balls would perhaps have sent the score up faster toward the end when the number of successive catches per trial at times exceeded two hundred. The essential course of the curve of progress would not, however, have been altered.

At the beginning the height of the room proved to be an important element, and the one in which the experiments were made was high enough to allow sufficient freedom in this particular.

The ball tossing proved itself an unexpectedly delicate index of bodily and other conditions. Slight changes in physical condition or in the temperature and illumination of the room often produced noticeable effects upon the score. This results in marked unevenness in the record from day to day, but does not influence unfavorably the general features of progress in which we are here most interested. Indeed, some of these disturbing elements are present at intervals in every learning process, and the importance of taking daily records of the progress in such work, instead of week by week, is apparent when we notice the great variations in skill through which most of the subjects passed.

The most frequent evidences of lack of skill in the earlier days of training were "wild throwing" (the tossing of the ball in such a way that it fell out of easy reach), and clumsy catching, *i. e.*, not being able to capture the ball when it touched the hand. As the subjects progressed somewhat, another source of failure appeared in the collision of the balls in the air, the ascending ball striking the descending one and knocking it out of reach. In the final stages trouble of this sort was again less frequent.

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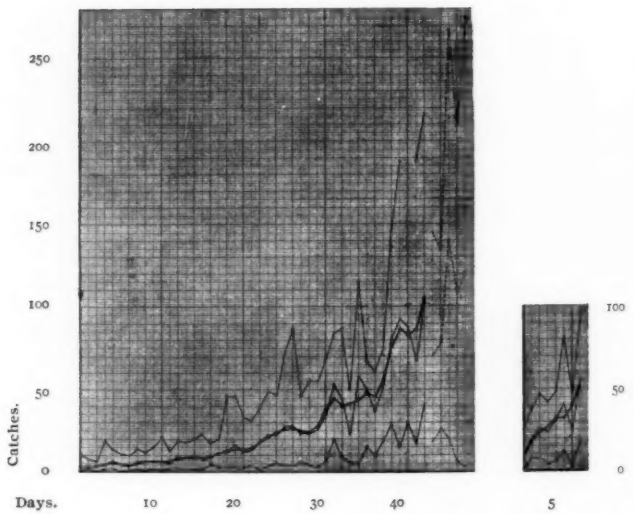
perform this operation twenty times without dropping one of the balls can treat the artist of the circus as a *confrère*." (Hopkins: Magic, p. 140.) Hopkins possibly means, however, a young man who *never* fails to reach at least twenty catches, however often he may try. If this is the case, he is speaking of a degree of skill which was hardly reached by my subjects.

CURVE A.



For all the Curves. The curve for the right hand is at the left. The upper, thin, line shows the highest single score (*i. e.*, the greatest number of catches made in a single trial before missing) for each day, while the lower, thin, line gives the opposite extreme, *i. e.*, the lowest single score for corresponding days. The thin line in the middle represents the daily average, and the heavy line that cuts across it is the smoothed average. The method of smoothing has been described above. (See page 210, foot note.)

The number of catches before missing is shown on the vertical axis and the days on the horizontal axis.

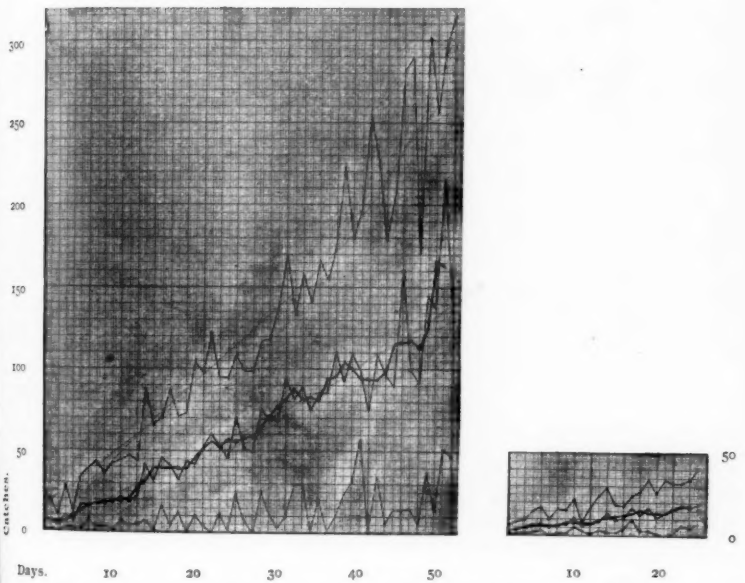
The "forgetting" curve is given in dotted lines. The three lines stand for the highest, average, and lowest respectively. Here the unit on the horizontal axis is thirty days. For the discussion of the results see page 221.

The curve for the left hand is at the right. The short line at the right of the vertical axis near its base and perpendicular to it shows the score made by the left hand before the right hand practice began. (See pp. 218-220.)

For Curve A. The break in the highest score, on the third day before the last, was caused by the subject being "called" every time at one hundred. He was trying to make ten scores of one hundred each.

Subject A played base ball when a boy but was only a mediocre player. He had never tried to keep two balls in the air until these experiments were begun. His method was to keep the balls in two parallel columns in front of him. He found himself doing this and then consciously continued it.

CURVE B.



For the general features of the curve see curve A under the heading "For all the Curves."

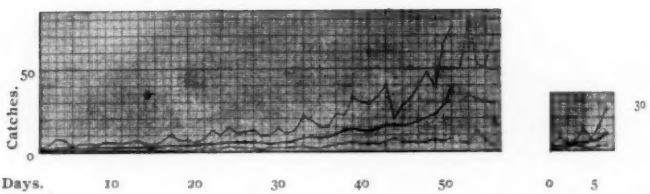
B played baseball when a boy and has since played tennis.

His steady rise without sudden jumps until the end is probably due to his effort to avoid consciously adopting a method of procedure. (See p. 215.)

His unusually high score on the next to the last day of his regular right hand practice was made during the April holidays, and on that day he was refreshed by a long walk into the country. The drop on the following (and last) day was accounted for by fatigue caused by working at his desk all the morning. (See p. 212.)

B did not take the monthly tests for determining the effect of intermission of practice.

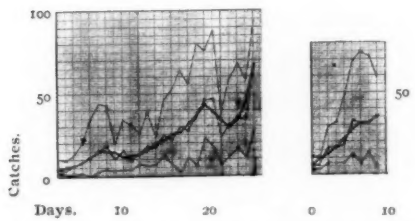
CURVE C.



For the general features of the curve see curve A under the heading "For all the Curves."

C never played baseball when a boy and he began his practice in ball-tossing by catching with his hands high in the air and the palms turned forward. His progress was very slow and on the thirty-third day he had evidently reached his limit by this method, the balls gliding down his hand before he could seize them. On the thirty-third day he consciously adopted a new method, holding his hand outstretched with the palm turned up. From this time he made a new start and his progress, though gradual, was steady. (See p. 214.)

CURVE D.

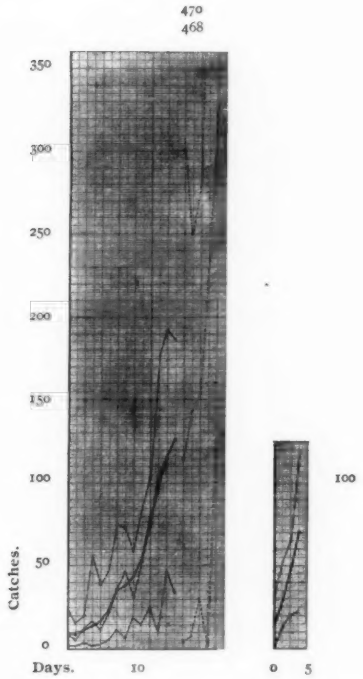


For the general features of the curve see Curve A under the heading "For all the Curves."

D was disturbed so frequently by sickness in his family that his curve is exceptional and shows the effect of loss of sleep on the learning process. He made regular progress during the first seven days and felt, as he said, that he had "the knack" when the effect of loss of sleep for several nights became apparent and he seemed to lose what he had gained. (See pp. 212 and 216-219.)

D did not take the monthly tests for determining the effect of intermission of practice.

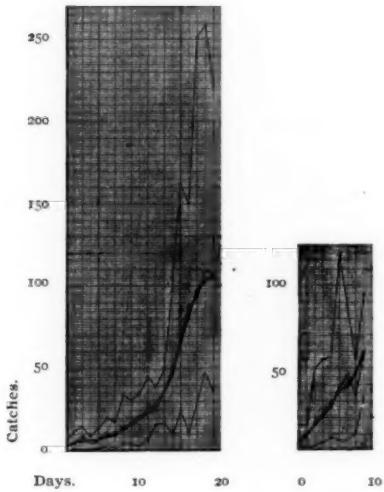
CURVE E.



For the general features of the curve, see Curve A, under the heading "For all the Curves."

E played baseball when a boy. In the tests he followed the plan of keeping the balls in two parallel columns in front of him. During the regular right-hand practice his scores were made with a great expenditure of energy, but after the regular practice ended and the monthly tests for determining the effect of intermission of training were made, his increasing skill became evident in the growing ease and grace with which he handled the balls. His highest throws on the last two monthly tests (the fourth and fifth month) were 468 and 470, while his average for these two trials was 229 and 337 respectively. Both of these averages, it will be observed, are far above his highest throw during the regular practice. During the last two "forgetting" months he took more or less exercise upon the tennis court. (See pp. 221-222.)

CURVE F.



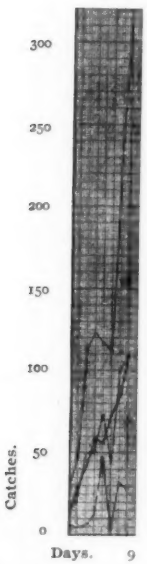
For the general feature of the curve see Curve A under the heading "For all the Curves."

On the third or fourth day of his regular right-hand practice F found himself tossing the balls up at nearly arms-length to the right and in such a way that they took a circular course and came down in front of him. The early adoption of a good method probably accounts in a large part for the uniform rise of his curve. (See p. 215.)

F played baseball when a boy but is not especially athletic.

F did not make the monthly tests of "forgetting."

CURVE G.



G is the curve for learning to keep three balls in the air with two hands. The subject had practiced handling two balls with one hand on the tennis court, but had never tried three.

For the general features of the curve, see Curve A under the heading "For all the Curves."

On the sixth day G dropped from 730 catches in ten trials to 431. When he began the day's practice he thought that he was in good condition and confidently believed that he would make his first thousand, but he found himself unable to control his muscles. What had been easy the day before now required the greatest effort and at the close of the practice he was in an uncontrollable tremble. (See pp. 212-213.)

On the last day G made over a thousand catches in seven trials and then stopped. The cross on the ordinate for that day indicates what would have been his average at the same rate for ten trials.

G did not take the tests to determine the effect of intermission of practice.

None of the subjects gave any preliminary thought to the manner in which they might best get the knack of handling the balls, and this led to individual peculiarities of method, and ways of avoiding or meeting the common difficulties. One subject found himself very early in his practice avoiding collisions by tossing the balls up a little to his right and in such a way that they would take a circular course coming down in front of him. This method was most successful in avoiding collisions. Two others fell into the way of tossing the balls up at nearly arm's length in front so that they took a circular course toward the subject coming down closer to his body. The objection to this method was that the balls tended to fall too near the catcher and so constantly crowded him back. The other two kept the balls in parallel columns a foot or so apart and a little to the right of the median line of the body. With this method the balls got into a "mix up" periodically and then the subjects were obliged to toss high until they could straighten out the tangle when they would settle down again into the same parallel columns, but only to have the experience of trouble repeated in course of time. The same was also true in a measure of the circular throws. We shall return to these personal differences later in discussing the individual features of the learning curves. For other matters bearing on influences that affect the score see the section below headed "Some Conditions that Influence the Learning Process."

In the execution the eyes and attention were upon the balls in the air, indeed, upon them in the upper half of their course. Both the tossing and the catching were executed by the hand alone, for the most part practically outside the field of vision and of attention.

Results. While full numerical data are at hand, it has seemed to the writer that the essential features of the results could be made more easily intelligible by diagrams than by tables, and he has accordingly plotted the accompanying curves. All the curves are plotted in the same way and upon the same scale. Vertical distances show the number of balls caught; horizontal distances the successive days. The main curves show the progress of the right hand, while the dotted portion, immediately to the right of them, shows the result of the "forgetting tests." The lower curves at the extreme right present the progress of the left hand. Points on the uppermost (thin) line show the highest score reached in any single trial on the day in question. The lowest line of all shows, in the same way, the lowest single score from day to day. The thin line in the middle gives the daily average. The heavy line gives a smoothed average.[1]

[1] The method used in smoothing was to average the averages for

Discussion of the Results. The curves just presented have certain characteristics in common.

1. With hardly an exception the curves are concave toward the vertical axis, which means, of course, that the progress was first slow and then more rapid. The curves which differ most from this type are those of B. In the case of his right hand curve the final rapid up-shoot is postponed and is not so conspicuous when it does come because of the steady, though gradual, ascent that has preceded it. His left hand curve was probably not carried far enough to reach the stage of rapid progress. It is altogether probable that all of the curves would in the end sweep more rapidly to the right and show a stage of slow progress as the physiological limit of skill in such matters was reached, but none of my subjects approached that limit. Bryan and Harter (6) found in their study of the acquisition of the telegraphic language a learning curve which had the rapid rise at the beginning followed by a period of retardation, and was thus convex to the vertical axis. The difference in form is very probably partly due to the difference in the type of learning involved, though it may also rest upon differences in the method of carrying out the test. This will be further discussed under the section headed "On Learning Short-hand."

2. All the curves show great irregularity of advance. Progress is never uniform but always by jumps. The learner seems to make no gain for several days or even longer, then he takes a leap perhaps to get a good grip and stay or may be to drop back a little. But if he loses his hold it is not for long and he soon makes this higher level his starting-point for new excursions.

A growing feeling of confidence usually preceded a permanent rise. The subjects felt that they were "going to do it."

There are not one or two special periods of delay in progress giving a "plateau" or two in the curve, as Bryan and Harter found in their study, in which successful co-ordinations are made automatic. Instead, there are many, the number varying with different individuals, and automatization is going on during the entire learning process. Miss Shinn found this same irregularity in the attempts of her little neice to learn to walk.

3. The average, holding at first somewhat closely to the lowest throw, is gradually drawn away from it by the growth in skill that reveals its reach in the highest throws. Though

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the first three days, then those of the second, third and fourth days, and, again, those for the third, fourth and fifth, etc., to the end. By this method smoothed values cannot be found for the first and last days and in strictness they should have no place on the smoothed curve, though they are connected with it in these diagrams.

the lowest throw does not rise much above those of preceding days the number of low throws continually decreases. The lowest throws are more frequently the result of accidental conditions than the highest. While the latter is always above the learner's usual ability at a given time, it none the less shows the direction in which he is moving, and its height on any day bears some relation to his rate of progress. That is to say, though the learner will not reach the level of a given highest throw on the following day, he will shortly approximate it and get there permanently very soon. The curve of highest throws may in a sense be regarded as a curve of amateur proficiency while the curve of lowest throws may stand as the curve of professional mastery.

Some conditions that Influence the Learning Process in Ball Tossing. It has already been mentioned that execution is greatly affected by physical condition. It is well known that physical experts of all sorts must keep themselves in condition, else they will drop into the class of inferior men. The influence of this factor was evident in all of the subjects. The exceptionally high score of 2,155 catches in ten trials reached by B on the day before the last was made during the April holidays, and on the morning of that day he had taken a long walk into the country. On the following day when his score dropped to 1,359 he had been working at his desk all the morning and did not feel fresh. Everything required greater effort.

D lost sleep so often after the first four or five days, on account of sickness in his family, that his curve came to represent the effect of physical condition on progress rather than the learning process. When exhausted from loss of sleep he lost the skill that he had gained before.

Sometimes the lowered vitality is not apparent to the individual himself. G, who threw three balls with two hands, made a score of 730 catches on his fifth day and when he began on the sixth felt confident that he would make his first thousand. But, instead of this, he fell to 431. After starting he found that he could not control his muscles. What had been easy the day before was now done only with the greatest effort, and at the conclusion of the day's trial he was in an uncontrollable tremble.

Probably the "off days" that all subjects had belong here. These differed from the days when they were simply unable to reach a high score of the previous day. Sometimes they felt no confidence in their power to do any sort of good work though they could give no reason for the feeling. At times "warming up" freed them from this feeling, but again even

the lower scores required much greater effort, amounting in some cases to an exhausting strain.

The correctness of the curves obviously depends upon the uniformity of the effort put forth by the subjects. It is commonly assumed that the maximum effort is a constant factor for a given individual and that the only thing needed to secure it in a conscientious subject is to interest him in the work and then ask him to do his best. While I have no reason to doubt the conscientious effort of my subjects and the practical uniformity of the average effort of each from week to week, the matter is perhaps of sufficient general importance to the precision of psychological experiments to justify a little discussion of it. Even a direct interest in the results of an investigation aided by the no less effective acquired interest aroused by the professional advantage from a well worked out problem will not enable the experimenter himself to make a steady maximum effort in something that has little mental content.

In the ball-tossing the influence of this element was less noticeable on account of its use of the voluntary muscles and because of the counter-effect of the subjects watching their own progress and of competing with others. But even here unintentional relaxation became evident now and then by comparison with the intense effort put into the work at other times, as for example, in the last half when the score of the first was found lower than the subject had hoped for. Anderson (1) found, too, that in strength tests every man but two "failed to equal his best record when tested apart from the other members of the class," and Johnson (15) observed the same thing in tapping experiments. Yet it is easier to hold ourselves to steady intense effort in feats of muscular skill and strength than in many other things because of our mastery of the voluntary muscles. The effect on the short-hand practice (see the second study of this series) was unmistakable and there can be no doubt that it influenced the curve in spite of every effort to resist.

This lack of energy, due to waning interest, probably has more to do with delaying the learner's progress and making "plateaus" than anything else. One cannot escape a dead level in uninteresting work and after the enthusiasm that novelty stirs has spent itself the interest is dulled and effort slackens. This is a potent cause of the long dreary period of no progress in learning to speak a modern language or to play upon a musical instrument.

But the slow progress is frequently only an apparent one and due to our inability to measure the advance. It is a case in which figures tell only a part of the truth. In the ball-tossing during B's slowest period, when the curve showed little or

no rise, it was evident both to him and to the experimenter that he was still making progress and the proof of it, aside from unmeasurable observation, was his occasional high throws. The curve remained stationary because his imperfect training had not enabled him to meet the chance emergencies that were constantly arising.

But the matter of interest is still more complicated. In ball-tossing, after one has reached a fair degree of proficiency, the first part of each trial is always something of a bore till the fifty mark is passed when it becomes interesting. Later the interest may take another slump, rising again after the score has reached one hundred. At this point the possibility of an unusually high score keeps the subject alert to the end. There was also a plateau in the interest of some of the men after throwing for the first time a hundred in a single trial, for which they had been very keen. They felt that it was impossible to reach the two hundred mark at once, the total thousand was too far off to be alluring, and so the edge of their enthusiasm was turned. Later when the chance to make another record seemed good they became as alert as ever. Indeed, after the satisfaction of having thrown a hundred had subsided, and the work continued for a time without great progress, the first twenty-five took on an acquired interest in the anxiety to get done. This brought greater care.

The whole question of maximum effort is well worth special investigation.

The confidence that follows a successful series of throws proved of considerable value, unless it led to the carelessness of over-confidence. Faith in one's ability to get out of a desperate situation in the tossing increases with success. This leaves the attention imperturbed, and one does not "go to pieces."

A long period of delay often represents the physiological limit with a particular method of tossing and a rise is made only by the introduction of an improved mode of procedure. This was especially noticeable in C who caught at first with the hand high in the air and the palm forward and almost perpendicular. This high catching seemed to be a sub-conscious accommodation to the position for throwing. The balls, of course, glided down his hand before he could seize them and he made practically no advance until he held his hand lower with the palm turned upward. This improvement, which necessitated the further change of keeping the balls at a distance from his body, was consciously adopted on the thirty-third day and at that time a new rise began. The general flatness of C's curve is doubtless due to the fact that he never played ball when a boy.

F, on the other hand, on the third or fourth day found himself tossing the balls up at nearly arm's length to the right and in such a way that they took a circular course and came down in front of his body. In this way collisions were avoided. The plan entered upon unconsciously was then consciously adopted, and as a result of finding a successful plan early in the work his progress was rapid and his curve is the most regular of the lot. It may probably be regarded as typical for muscular feats in which there is no long continued feeling around for a successful way of doing the thing, as when the learner is assisted by a good teacher.

B, again, tried pretty constantly, throughout, not to consciously adopt any method, but to let everything take its natural course and this seems to be the reason for his slow but steady ascent without any high jump until near the end. His efforts in this direction did not, however, prevent his final approximation to a regular method though one less advantageous than that developed by F.

We see in this the value of suggesting good ways of doing things while the learning is still in its early stages. If the learner goes on he will finally develop a plan of his own but only after a good deal of wandering and even then it may not be the best. But the suggestion to be effective should be given at the time when need for it is keenest, at the "psychological moment." It is then that its value will be felt.[1]

In polo, golf, baseball or football good form is absolutely necessary for reaching a high degree of skill. It is the essential prerequisite for good methods. Movement and position become associated and a change in the latter requires relearning the former. The physiological limit of bad form is low.

In learning any complicated performance, we progress by sections. That is to say, the co-operating movements improve separately. This leaves certain errors conspicuous when we are well along in the work. Indeed the whole learning process, at least in learning to toss and catch balls, seems to consist in eliminating errors. First the obvious ones are gotten rid of, then new ones appear, and it is only after all have been overcome that the thing can be regarded as mastered.

In avoiding errors there was adaptation, apparently more organic than conscious, to conditions and often it was so delicate as to elude observation. B, for example, found himself tossing high in order to have time to recover from a difficult situation, and at another time he caught himself putting his body into a more alert position by slightly raising himself on

[1] Miss Shinn reports that her neice until six years old always ran flat-footed, but when she was shown the advantage of keeping her heel slightly lifted, she readily adopted it.

his toes and making his muscles tense. Then he realized that he had been doing it for several days. So far as he could determine consciousness had no originating part in it.

It is interesting that all the subjects improved by hitting upon better ways of working without any further conscious selection, at first, than the general effort to succeed. There seems to be a competition of methods. Just how this selection occurs without conscious interference is not easy to say. Consciousness discovers modes of action already in use and selects some of them for survival because of their success. They then pass into the automatic. In this way reflex movements may have first been conscious.

Two learned to throw in less than half the time that the others needed, but their movements always called for a great outlay of energy. Economy of effort is an important element in effectiveness, but its getting requires time for the solidification of associations and for the elimination of useless movements, with the subsequent automatization of just those that are essential to the process.

A certain amount of "warming up" was usually necessary. While high throws were not confined to one part of the day's trial, they rarely came at the beginning. Commonly, so long as the score was low enough to eliminate the effect of fatigue, the one or two high throws after the warming up period were followed by a slump which again yielded to high ones toward the end. This form of the daily curve was too common to be entirely a matter of chance. It is another case of the uncontrollable variation of the maximum effort.

In his experiments on Practice and Habit, Johnson (14), also, noticed that his subjects could not get control of their muscles within the time of the preliminary tests.

Bryan and Harter have told us that it is intense effort that counts, and this is true, but with a qualification. Throughout this investigation it was clear that attempts to spurt were not effective. Indeed, the very effort interfered with success by making the attention too obtrusive. Special strain is itself a distraction, as Johnson found it at times. It is steady and calm intensity that counts for progress.

The importance of strenuous effort lies in the fact that up to a certain point of intensity it is generally *successful* effort and that is what counts, as Woodworth (25) and Johnson (14) found.

Fatigue from any cause not only brings a lowering of the day's score but the entire process of learning is probably hindered. The growth of the nervous system into the required forms of activity has been disturbed.

D felt that he had caught the knack when sickness in his

family brought loss of sleep, and it was nearly a week before the feeling of confidence returned. Meanwhile his score dropped, except for occasional spurts, far below that which we should expect from his previous and subsequent record or from that of others.

F, too, felt that he was delaying his left hand progress by practicing when fatigued from lectures, and a change of hour brought immediate results.

In tossing and catching the ball a pretty general co-operation of all the muscles of the body is required, though those, of course, that effect the movements of the arm and hand are most directly involved. Of these the movements most prominent in consciousness are the general movements of the arm. The body movements, in most cases, do not come into consciousness at all, and the finer movements of the fingers and hands, except so far as they are covered by the general intention to toss and catch the ball successfully, are almost equally unregarded. As a matter of fact, however, skill in throwing and catching is rather more an affair of the fingers than of any other members.

The question then arises how are the necessary co-ordinations brought about? It does not seem difficult to bring the matter into line with phenomena already pretty well known. Let us suppose a successful toss and catch is made. This is followed by a double effect; it leaves, as every action does, a trace in the nervous system which facilitates later repetition of the same action, and the successful adaptation also gives rise to a feeling of pleasure. The effect of pleasurable sensation is a heightening of muscular tonicity or a general tendency to motor discharge, which in the case of an action just performed—one whose neural effects are still lingering—is equivalent to a partial reinnervation of the same co-ordinated group of muscles which again deepens the existing trace. The next actual effort finds the nervous mechanism a little readier to react in this favorable way. In case of an effort that does not lead to success, the slight displeasure at failure exerts its natural depressant effect upon the whole neuro-muscular system, and this does not deepen the neural trace left by the original movement, and even, perhaps, breaks up the incipient co-ordinations that gave it its particular form. In any case, whatever its mode of action, it has not the reinvigorating effect upon the original neural trace exercised by the pleasurable sensation, and therefore, in the long run, the successful movements, and the co-ordinations upon which they depend, tend to persist while those that are unsuccessful tend to fall away.

Now it will be observed that this action of pleasant and unpleasant sensation does not depend at all on consciousness of the detail of the movements and applies as well to a movement

of which all the ultimate factors are unconscious and only the general end known. In such a movement, if the result is unsuccessful, a slightly different movement follows at the next trial, that is, one in which the co-ordination among the muscles engaged is slightly altered, as an automatic result of the partial inhibition of ill success. This new trial may be no better than the previous one, in which case it is again altered until success is reached or the attempt given up completely. In the case of movements the details of which come more or less completely into consciousness the same process goes on but with more rapid progress toward the desired end, because the variations from which the advantageous movements are selected are not chance variations, but are from the start more or less perfectly suited to the requirements of the case. In the ball tossing the general arm movements remain the prominent thing in consciousness, as we have said, while the finger movements are little noticed or quite neglected, and yet, nevertheless, the whole co-ordinated group is worked over, under the influence of the voluntary movements, into proper adaptation for the successful performance of the feat.

2. *The Effect of Right Hand Training upon the Skill of the Left Hand.* As already described (section above) the subjects in this investigation were all tested with their left hand before the right-hand practice began, in order that the effect of the latter upon the former for the left-hand tests, which stand at the right side of the charts given above, might be determined. In the curves the short straight line projecting to the right of the vertical axis shows the score made by the left hand in this preliminary test. The progress of the left hand in its subsequent practice is shown upon the same scale and in the same way as that of the right hand by the light and heavy lines of the diagrams.

Several things are at once noticeable.

1. The record of the first day of regular left hand training is in all cases higher than the preliminary test, though in no case had the left hand been practiced with the balls at all during the interval. More than this, the score never drops to the level of this preliminary test, which shows that the gain was permanent.

2. The left hand curves bear a striking resemblance in general form to the corresponding right hand ones with this difference that in all but one case they ascend much more rapidly. A did in eight days with his left hand what his right hand needed thirty-eight days to accomplish. E made a left hand record in four days that he had not been able to do in less than eleven days with his right. The difference with the others is not so marked, but in all cases but one the left hand curve rises more rapidly than the right.

3. All of the subjects but one made a better score with their left hand on the first day of its regular practice than they had been able to do with their right at the beginning of the work.

4. The highest and lowest left hand single throws are in almost all instances higher than corresponding right hand throws on corresponding days.

F was delayed in his left hand progress at about the middle of the work by physical and mental exhaustion. The anomalous record of B will be considered a little later in another connection.

The conclusion is unavoidable that in the majority of cases the training of the right hand was somehow effective upon the left also. The same general result has been noticed by many observers engaged in different lines of investigation, and has been made the subject of a special investigation by Davis.[1] The chief point of interest is to discover how the effect is produced. Is it due to some purely peripheral change, or to some alteration in the central nervous system, or finally to some method or plan of work that can be applied equally well in the case of either hand, as for example the knowledge of spelling which a man could use as well in writing in mirror script as in the ordinary way? It is not impossible that cases could be found that would exhibit the co-operation of all three. In the ball tossing there was evidence, certainly, of the last two. All the subjects were able to make use with the left hand of the methods of handling the balls, and of recovering control of them after an ill-directed throw, that had been developed in the right hand practice. In all the cases but one a good deal of a less conscious facility (of a sort that might indicate some kind of symmetrical training of the central nervous system) was probably present. In the case of B, whose record supplies the anomalous case in the left hand training, the "method" of tossing and managing the balls was distinctly carried over, but the less conscious neural habits were apparently not present. He could get out of difficulties with remarkable dexterity, but he failed in the simple things. He could not use these "higher habits" to the best advantage because he did not have the lower, and in his case these came with more than usual difficulty. The others were able at once to build in the sub-structure of central (or neuro-muscular) skill and so to learn the art of left hand throwing much quicker than the right. The mental element, the power to comprehend and meet a situation, is evidently, then, in most cases, the more difficult part of complex muscular feats of skill, since the right hand, if taken first, needs so much more time for the learning than the left,

[1] See 9 (bibliography) also 10, 11, 18, 20, 21, 22, 23 and 24.

notwithstanding its greater general facility in such movements in right handed people.

The anomalous nature of B's left hand curve led to the inquiry as to whether he was relatively less skillful with his left hand than the other subjects. For testing this a target approximately seven feet six inches in diameter, with nine concentric circles each about four and a half inches wide, was used. The target had previously been used by A. W. Trettien. The test consisted in throwing one hundred balls with each hand from a distance of thirty feet. The bulls-eye was nine inches in diameter and the balls used were such as have been described above in the section on ball-tossing. The following shows the success of the left hand compared with the right in percentages. C = 45 per cent., B = 50 per cent., F = 66 per cent. while A and E each gave 72 per cent. It is evident from this that, with the exception of C, B was relatively less skillful with his left hand than the others. C did not continue long enough with his left hand in the regular work to enable us to say just how his curve would finally have compared with that of B. If now we consider the skill of the right hand, merely, estimating it by success in throwing at the target, we have the following order, beginning with the best. F, E, B, C and A. Again, arranging the subjects in the order of their left hand skill, leaving the right hand out of consideration, we have, beginning as before with the best, E, F, A, B and C. It is evident from this that left hand deficiency is, at any rate, one cause of the anomalous form of B's left hand curve.

To return, now, to the subject of left hand training, it would be a mistake to suppose that such experiments in cross-education give support to the doctrine of "formal education." There is no evidence to show that training has general value. Indeed it all argues strongly for the influence of content. Volkmann (22) found that six months of regular practice in distinguishing small visual distances in which his eyes gained remarkable power, had no effect whatever on his ability to perceive small tactual differences. The right hand has had a great variety of training that ought to bring it along rapidly in ball-tossing on the principle of formal training, but this investigation shows just the reverse. The right hand learns it very slowly but the special training that comes from doing it enables the left hand, awkward and stiff as it is, to get control of the situation in about one-third of the time required by the right. Skill in certain lines may be serviceable in other similar processes, but its value decreases as the difference between the kinds of work increases, and in many cases it is probably reduced to zero.[1]

[1] For a full discussion of this matter, with experiments, see the work of Thorndike and Woodworth (26) and Bair (3).

3. *The Effect on Skill of the Intermission of Training.* To find out how rapidly a feat of muscular skill could be forgotten three subjects were tested with their right hand one month after they had finished with their left and the test was repeated every thirty days for five months. The curve is given in the chart by the dotted lines between those for the right and left hand.

Instead of being a forgetting curve, it turned out to be a new curve of learning though the subjects did not touch the balls during the intervals.

During the second and third month E rose steadily in skill, his average and highest throws greatly exceeding those made at any time during the regular practice, while in the fourth month his average exceeded his highest previous throw and in the fifth month he made the astonishing average record of 337 catches in ten trials. In this test his lowest score was 135 catches, and, with the exception of this and one of 196, none of his scores fell below 300 while three exceeded 400 catches. A began his rise the third month, and with him, too, the highest throws were far above those made during the regular work. C's score just about held its own with that of the regular practice. Evidently the mind not only grows to the modes in which it is exercised but this mental growth may continue, for a time at least, after the practice has ceased.

Prolonged practice in any muscular exercise brings increase in the size (or efficiency) of the muscles as well as increase of skill in their use. This portion of the ball-tossing study gives evidence of the relative independence of the two. The point at which fatigue came during the regular practice, varied somewhat, of course, with different subjects, but it was first felt after about forty or fifty throws. If the subject recovered from this, he felt comfortable until he approached ninety or one hundred, when fatigue came for the second time. If he pulled through this, he was all right for another fifty, at least so far as fatigue was concerned, and so periods of vigor and fatigue alternated about every fifty throws. But during the experiments in "forgetting" fatigue came on earlier and at times was so exhausting that the subject could not continue. But skill during this time, in which muscular endurance decreased so rapidly, suffered no decline. Indeed, in two of the three subjects it greatly increased during the thirty day omission of practice.

That fatigue prevented us from seeing the full increase of skill during these months in which practice was omitted is evident from the fact that E made his remarkable series of the fourth month when fatigue was much less noticeable than in the three preceding tests, because he had been playing tennis

daily during the previous week. Then, again, in the interval between the tests of the fourth and fifth months he played tennis quite regularly and his unprecedented score at the last of these trials, five months after the end of the regular practice, shows a further increase in skill during the intermission of training, notwithstanding his loss in muscular endurance previous to exercising on the tennis court.

That there should have been little loss is not very strange, for it is well known that feats of bodily skill, like swimming, dancing or bicycling, are not wholly lost by long periods of disuse, but we were not prepared for a positive gain in skill. It is interesting in this connection to cite Houdin's (13) experience who tells us that having in the past learned to handle four balls while reading he was still able to keep three in the air and read at the same time though he had "scarcely once touched the balls during the thirty years preceding." And in *L'Année psychologique* (5) Bourdon reports experiments showing that there was not only no loss in skill in mental processes after an interruption of the training for a period varying from twenty-eight to thirty-eight days and longer, but in most instances there was a positive gain during the intermission, while facility in certain mental processes was not wholly lost after seven or eight years' omission of practice.

Summary of Results and General Inferences.

1. The curve for learning a feat of muscular skill, so far as this study may be regarded as typical, is concave toward the vertical axis. (See p. 211.)

2. Progress is never steady but always by jumps, with not one or two but many intervening periods of delay. (See p. 211.)

3. Practice with one hand trains the other, as has already been observed by earlier investigators. (See pp. 218-220.)

4. The gain seems to be due in part to the possibility of the transference of many points of "method" and their application to the throwing of either hand and in part, also, to a more direct effect of training, probably upon symmetrical portions of the nervous system. (See pp. 219-220.)

5. Method was hit upon and improved by the subjects of these tests at first without conscious intent.

6. The general physical condition of the subject greatly influences his skill in ball-tossing as well as the effectiveness of his practice from the point of view of acquisition. (See curves G and D and p. 212.)

7. Maximum effort, in spite of conscientious intentions to the contrary, is a variable standard, and emotional factors of various sorts probably affect the score and by inference the learning process as well. (See pp. 213-214.)

Pedagogical Hints and Suggestions.

It would hardly be possible for one interested in education to carry through a study of this sort and not bring away a plentiful crop of pedagogical hints and suggestions, some, perhaps, mere analogues of little worth, but others that point the way to interesting lines of investigation. Without attempting to defend or enlarge upon any of them the writer may set down the following.

1. Growth in muscular skill stimulates intellectual development in the final determination of the method. We get into ways of doing things unconsciously, find ourselves doing them, in fact, but later those that survive do so for a reason. This is important in the development of children through play.

2. The effect on the experimenter of watching the lowest and highest throws is interesting. Teachers are apt to estimate a good pupil by his best achievements, forgetting his comparatively few failures, while a poor pupil is estimated by his poorest work. The best work is impressed upon our mind in the one case and the poorest in the other. It has the same psychological basis as the saying "it always rains when I do not take an umbrella."

3. Monotony.

It is a platitude, which nevertheless must continually be dinned into the deaf ears of schoolmasters, that children do not see things as adults do, and to them future benefit is at best a poor recompense for present misery. If remote interests often count for little with adults they surely have even less force with children.

4. The effect of fatigue during the learning process has not received the attention that it deserves. Most of our tests of fatigue on school children, so far as they relate to work, have been made in connection with what the child had learned long before and which had become automatic.

If, as Woodworth (25) found and as these experiments seem to show, it is not mere practice but successful practice that counts, then school work continued to the point of fatigue is disastrous.

5. The influence on their general progress of watching their own advance from day to day was undeniable in the case of my subjects and it suggests the question of introducing it into school work. As it is now children are always tested by new demands or by comparison with companions who are going along with them. On this account they often feel that they are making no advance, because their marks do not show any, but if they could see their own curve grow from day to day they would not only be interested in its variations but would also be convinced of their progress.

6. Children evidently cannot be expected to make even slow continuous progress. They must have time to catch up with themselves, so to speak. There is evidence all through this investigation that there are moments when we run ahead of our power and delays are necessary that associations and habits may have time to set.

7. Bourdon's experiments, as well as this investigation, raise the question of how many recitations per week in a given subject will be most advantageous for children. It is by no means certain that one each school day brings the best results. They should, of course, be frequent enough to prevent loss of interest but also far enough apart to give sub-conscious processes a chance. Special investigations are needed here.

8. Periods of retardation may represent the physiological or psychological limit for the method used.

9. The fact that attempts to spurt instead of making for progress delayed it by bringing into prominence psychical activities that serve the learner best while in the background, counts strongly against cramming.

10. Suggestion of a good way to do the thing saves time that would otherwise be lost, but the suggestions must be made when the learner feels the need for them.

II. On Learning Short-Hand.

When short-hand was selected to illustrate the more strictly mental side of learning, it was thought that this might give a typical curve and that, in any case, it would furnish an interesting parallel to the research of Bryan and Harter on the learning of telegraphy already referred to. The practice and tests, however, seemed to show that each branch of knowledge has its own characteristics which so greatly affect the learning processs that we can speak of a typical curve of learning only in a very general way.

For this test but a single subject (the writer) was available. An hour and a half each day was given to the study and practice of short-hand and the study was continued through something over ten weeks. During the first half the study time was spent in writing, but in the latter half it was about equally divided every day between practice in writing and in reading what had just been written.

The subject was tested daily, at first in writing only; later in both reading and writing. In the writing tests James' Talks to Teachers was used for dictation. There is no doubt that his rich vocabulary had an effect upon the curve. Had some author with a less extensive range been taken it is probable that a more rapid rise would have commenced at about the second third and the ascent then have proceeded with continu-

ally increasing rapidity till the limit was approached, when it would have passed over into the usual physiological plateau.

An assistant read a few words, usually a phrase, then waited until, by pronouncing the last word, the subject indicated that he was ready for more.

The subject wrote as fast as he could for a period of ten minutes and his score for the day was then measured by counting the words and also the lines and parts of a line in order to equalize, so far as possible, the effect of long and short words.

The tests in writing began February 18th and were taken each day until April 4th after which they were omitted on Sundays. They closed April 30th. The reading tests did not begin until March 8th and there were two omissions, March 10th and 27th. In other respects they corresponded with the writing tests.

The method at first adopted in the reading tests was for the subject to write down the words in long hand as he read them. The length of the test period was, again, ten minutes but only ten seconds were allowed for a word. If the subject did not indicate within that time that he had the word time was called and he was required to pass on to the next.[1] At the end of the ten minutes his written work was compared with the text and the number of words and lines correctly read was recorded. When about two-thirds through, however, the limit by this method was reached on account of the time taken in writing. This point is indicated by the break in the curve. On the eleven following days we alternated between the plan of having the subject write the words as before and that of having him read them aloud while the assistant followed in the text noting the mistakes. After that the latter method was followed to the end. The curve for translating without writing is shown in dotted lines in the diagram below.

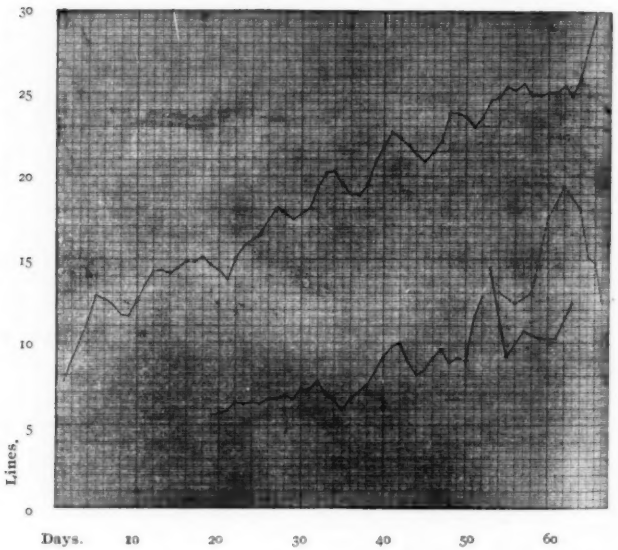
The material for the reading test was that which the subject had himself written ten days or more before, and so it had the natural difficulties increased by poor and sometimes incorrect writing. It was fully "cold" and only once or twice was the reader helped by memory and then only in one or two sentences by the general idea. The Pernin system of short-hand was used and no attempt was made to abbreviate by phrasing, as that would have introduced a new element.

The upper (writing) curve is smoothed by threes as before (see foot note p. 210) but that for reading is smoothed only to

[1]The time was not always called with perfect regularity. An ordinary watch was used and the assistant found it difficult to carry the task through the full ten minute period without slips, but the average time was pretty fairly kept and the differences due to this cause are probably insignificant.

the break. From that point the curve is drawn from the records for each day.

CURVES FOR LEARNING SHORT-HAND.



The curves for learning to write short-hand is above and that for reading the symbols below. The number and parts of lines written or read are shown on the vertical axis, the days on the horizontal. The break in the reading curve on the thirty-fourth day indicates the time when the subject began to alternate between writing the words in long hand as he read them, and reading them aloud while an assistant followed in the text noting the errors. From this point the curve gotten by the former method is given in the unbroken line as before, while the dotted line shows the progress when the score was kept by the latter method. (See p. 225.)

In the curve for writing (the upper one), the first rise from complete inability is rapid because the acquisition of a few imperfect co-ordinations and associations is easy and helps enormously when one starts at the zero point. Very soon, however, the subject makes all the gain possible of this rapid sort and then the rise settles down to a more or less gradual ascent. This curve would naturally outstrip that for reading at the start because the writer knows what he is to say and is not delayed by wrong translation of a preceding word, as in reading. Relieved of these embarrassments the writer in short-hand

and, probably, the sender in telegraphy, can give his whole attention to getting off the word before him.

After the force of this initial rise is spent, retardation alternates with progress. The difference between this beginning rise and the periods of lesser gain that follow is that a little gain makes more showing at the start. There is no evidence furnished by this investigation of a special time for the formation of "higher habits," or association groups. When there is continued arrests in progress its sufficient cause, in the writer's opinion, is to be found in the emotional factors of learning. This will be further discussed below.

In ball-tossing the initial rise was practically absent, and in learning short-hand it lasted for only a few days.

Bryan and Harter (7), in the receiving curve for telegraphy, found a rapid initial rise followed by a period of little or no progress, or, as they term it a "plateau," and they conclude that "learning to receive the telegraphic language consists in acquiring a hierarchy of psychophysical habits" and that "a plateau in the curve means that the lower-order habits are approaching their maximum development, but are not yet sufficiently automatic to leave the attention free to attack the higher-order habits."

The nearest parallel in my work to telegraphic receiving is the reading of the short-hand notes. A glance at the chart will show that nothing of the initial rise is to be found in the curve for reading and that there is nothing also that looks like a "plateau." The short-hand writing is more nearly comparable with the telegraphic sending, the curve for which as given by Bryan and Harter[1] shows a rapid rise at the beginning, but no such rise is apparent in my writing curve except for the first few days while the symbols were being learned. (4.)

Of course the amount of daily practice in ball-tossing, as managed in my experiments, increased as skill was gained. The learner threw more balls before he missed. But this increase of practice with growing skill is characteristic of all learning. A beginner in short-hand, and in telegraphy also, gets in more practice to the hour the second week than the first. So the ball-tossing was not wholly exceptional in this respect. In order, however, to see the effect of equal amounts of practice, A's curve was replotted in such a way as to show the progress per thousand throws. Even this failed to show convexity.

[1] None of the curves for learning to toss balls, as already noticed, have the initial rise with the succeeding "plateau" that characterize Bryan and Harter's curves.

The writer does not wish to dispute the gradual formation of such a hierarchy of habits as Bryan and Harter have described, nor to call the accuracy of their work in question, but he believes that the explanation of the order and the method of formation of such habits is different from that suggested by these authors and in particular that the "plateaus," when they occur, are to be accounted for in other ways. This immediate rapid rise at the beginning seems to be true only of these things that have symbols or other devices for handling and presenting ideas, and it is probable that after this first spurt, the length of which would vary with different sorts of material, the general form of the curve for learning is concave until the physiological limit is approached. Telegraphing involves fewer symbols, and the distraction of deciding on sounds and abbreviations, that mark the learning of the Pernin short-hand system, would not so greatly disturb the beginner, and so, having less thinking and deciding to do at the start, the learner in telegraphy would probably go on improving without great set backs longer than the short-hand writer.

In reading short-hand it was found that the speed rose with great rapidity whenever the reader was able to get the context. So long as he read by words, every word had to stand for itself, but when he got control of enough words to give him the sense it was no longer necessary to recognize each word. He could even correct in the reading those that were wrongly written.

The effect of getting the context was seen in occasional spurts soon after the reading tests began. Later they became more frequent.

The material for testing was taken as it chanced to come in the book, and the sudden drop in the reading curve at the end was due to its unusual difficulty.

In learning short-hand, and presumably also in learning to receive or send telegraphically, a large number of associations are formed that do not affect the speed of work, because there is no opportunity to use them and the learner seems to make little or no progress, not because this is the particular time for the formation of a "hierarchy of habits," for this is going on all the time, but because the range of association knowledge in the subject is too limited to meet the demand. After enough has been accumulated to meet the demands of the tests the curve will rise more rapidly. The associations that have been forming from the start have now become numerous enough to be effective.

The essential thing in getting a purchase here is to learn enough to make the associations already formed available. When this is done the rise comes.

That the failure of this material to influence the curve earlier is not altogether due to the lack of higher association-habits is evident from the fact that whenever the context is gotten, symbols that had long lain unused and were almost forgotten come in for service with the rest.

Associations are more active during the periods of delay only in the sense that there is more material to work upon than before, since they are not present until after the first rise that accompanies the rapid gathering of symbols at the start, but this is true of any later stage in the process when compared with an earlier one.

Bryan and Harter (6) tell us that in telegraphing "the learner enjoys the practice of sending, but feels practice in receiving to be painful and fatiguing drudgery." Unfortunately this feeling cannot be gotten rid of by setting aside certain periods for work. Practice is less effective and ennui may reach such a degree that little or nothing is accomplished during the hours of work. The effect of this monotony could be seen in the ball-tossing, but it was less influential there for reasons already given.[1] In the hours of practice upon the shorthand, however, its influence was great and wholly uncontrollable. The lack of content made the material so dry that the attention would wander in spite of every effort on the part of the subject to make his periods of practice equally valuable.

I can hardly doubt that this emotional factor was largely influential in making the plateau that Bryan and Harter found. Visible success is always interesting to the one who succeeds, but after the first dash has been made, and the easy acquisitions have been gathered in, the rest of the work involves a good deal of drudgery and all the indirect interests that may be brought into play cannot wholly relieve it of this drag. The beginner in German is fully conscious of the initial rise and thinks it great fun to say "*Guten morgen*" and "*Wie befinden Sie sich heute*" and his progress has certainly been great and rapid when compared with the zero with which he started, but the fun ends and the depressing monotony weigh him down long before he has reached the point where he can understand a native.

The wearying monotony and discouragement of this intervening period lessens the efficiency of the work which was so effective during the first spurt. Then, when the learner has reached the point where he can get the context and guess successfully, his practice increases without conscious effort and the pleasure of success causes him to redouble his efforts.

Psychical adaptation, too, greatly affects the curve and it is

[1] See p. 213 above.

probably this almost unconquerable tendency to remain in the stage that just meets our needs, rather than the lack of higher association-habits, that makes it so difficult for an operator to rise above the skill necessary for his own office work and that prevents years of daily practice from bringing a man to his own maximum ability to receive. Johnson (14) also thinks that Bryan and Harter's plateaus are "resting places in the effort," and in a large measure this is probably true.

Summary. Some points of agreement in motor and mental learning may be set down here by way of summary.

1. As in ball-tossing the learner seems to make no advance for a time and then springs to a higher level, perhaps only to fall back a little but, at all events, not to go higher until he has strengthened his position here.

2. I have found no evidence for one or two special periods of delay in progress in which preparation is made for a higher order of habits. As before, automatization is going on throughout the process.

3. Here, too, consciousness discovers certain methods in operation and approves or disapproves of them. So improvement goes on through elimination and selection.

4. The effect of variations of maximum effort was apparent also in the tests with short-hand, and here the periods of discouragement did not always coincide with the 'plateaus.' Several times the subject was confident that he had beaten his record until the count proved that he was still on his old level.

5. Physical condition was a no less determining force than before. Many times the drop could be clearly traced to it.

6. As with the balls over strained attention was a hindrance. Signs that were gotten incidentally and used occasionally, without special effort to fix them, were remembered better than those upon which a good deal of effort was expended. Effort defeated its own end by calling into consciousness other similar signs.

Symbols just learned, and which could be used with considerable facility when the subject was alone, could not be recalled under the tension of the test. The very fact of being tested had an inhibiting influency on the mental processes.

The pedagogical bearing of these observations is obvious.

III. On the Origin and Control of the Reflex Wink.

The experiments on control were the first undertaken and it will be convenient to consider them here.

Darwin tells somewhere of standing before a cage of snakes with his face close to the glass and finding himself unable to resist the instinctive tendency to protect his eyes from harm by winking and jumping back when they struck against the glass,

and that, too, though fully conscious that the plate glass was thick enough to absolutely remove all danger.

Since the wink is a protective reflex of the greatest importance for the protection of sight in animals with movable eyelids, and so for their preservation, it is of no little interest to determine under what conditions it is amenable to the will. Some of these conditions had already been investigated by Partridge (17).

In the following experiments which were directed to a more detailed study of the means by which control of the wink is learned the subjects were, again, university students and one professor.

The apparatus consisted of a framed piece of plate-glass about six by eight inches in size which was attached to a steel rod supported on four legs. A small wooden-headed hammer was attached to the lower part of the frame behind the glass in such a way that when it was pulled down and released by the experimenter it would fly up and strike the plate glass. The wooden head of the hammer was covered with cloth and a strip of rubber so as to reduce the sound somewhat and lessen the danger of cracking the glass.[1]

In the first series of experiments the stimulus was both visual and auditory, no further effort being made to lessen the noise caused by the blow of the hammer or the rattle of the apparatus than that described above. The subject sat with his face close to the glass and his chin resting on a frame support. Effort was made to keep from winking but there was no strain of the attention or muscles. In order to determine whether the period of the day had any noticeable effect, tests were made in the morning before the subject began his work and again at noon and, finally, they were repeated late in the afternoon just at the close of the day's work. They were continued under these conditions for five days.

Results. The time of day and such fatigue as comes from an ordinary day's mental work had no appreciable effect. Three subjects tested this by reacting in the morning, before the day's work, at noon and finally late in the afternoon.

First day { 1st Series of 20 trials—W = 9, P = 0, I = 0.
2nd " " " —W = 14, P = 0, I = 0.

N. B. Throughout these experiments W = full wink, P = partial wink, *i. e.*, the least noticeable eye movement in response to the excitation and I = inhibition of the wink. After a little practice the experimenter was able to distinguish four degrees of winking, depending upon the force of the movement and completeness of the process. It

[1]The apparatus was at first the same as used by Partridge, but as the experiments progressed it was altered from time to time in minor details.

has seemed best, however, to consider only the full wink and the least noticeable reaction to the stimulus.

Fifth day { 1st Series of 20 trials—W = 2, P = 7, I = 3.
2nd " " " —W = 1, P = 12, I = 3.

A series of artificial devices for distracting the attention was then tried with the same subject. Some of these diversions were winking fast between the strokes of the hammer, biting the lips and refraining from swallowing the saliva until it became annoying. These distractions were very effective when first adopted but soon lost their power. The following are samples of reactions made on different days under the influence of varying distractions. The entire series occupied seven days. Distractions of this sort were rarely effective longer than two days.

20 trials—W = 0, P = 17, I = 2.
20 trials—W = 0, P = 5, I = 12.
20 trials—W = 0, P = 3, I = 17.
20 trials—W = 0, P = 3, I = 17.
20 trials—W = 1, P = 7, I = 12.

At this point a new subject was taken and he was instructed to put his attention on inhibiting the wink but to avoid any muscular strain. As there are great individual differences it does not seem best to compare the reactions of different subjects but rather to follow the attempts at inhibition of each subject under varying conditions.

As before, the following are the reactions for the first day. The tests were made in the morning, at noon, and late in the afternoon.

1. 20 trials—W = 7, P = 3, I = 0.
2. 20 trials—W = 1, P = 15, I = 1.
3. 20 trials—W = 5, P = 11, I = 0.
4. 20 trials—W = 0, P = 10, I = 4.
5. 20 trials—W = 7, P = 4, I = 3.
6. 20 trials—W = 1, P = 10, I = 8.

Six days' practice brought no improvement. The result on the last day was

1. 20 trials—W = 4, P = 2, I = 2.
2. 20 trials—W = 0, P = 9, I = 5.

The same subject now tried the effect of focusing his attention on a point on the wall behind the hammer and directly in its path. In another series he thought of some just discernible dots on the wall at the same time keeping his eyes fixed upon them. The purpose was to withdraw the attention from the excitation. The improvement over the reactions when the attention was on inhibiting was marked. These experiments, which occupied four days and included two hundred and

eighty excitations in series of twenty each, caused a total of only fourteen full winks. The following are samples taken from the first and last days.

1. 20 trials—W = 5, P = 12, I = 2.
2. 20 trials—W = 0, P = 7, I = 13.
3. 20 trials—W = 0, P = 13, I = 5.
4. 20 trials—W = 3, P = 6, I = 5.
5. 20 trials—W = 1, P = 19, I = 0.

It may be mentioned here that very little improvement was observed so long as effort was directed merely to inhibition. Control seemed to come in proportion as excitation and inhibition were forgotten.

A series lasting four days, in which the attention was given to the thought of keeping his attention on a black target behind the hammer, gave practically the same results as the series that we have just considered.

The effect of reducing the sound by closing his ears with his index fingers was now tried. Series with the ears open were alternated, but with some irregularity, with those in which they were closed. Six days were given to these and the following show the results of the first and last. The attention in both sets was again on the thought of keeping his attention on the black target behind the hammer.

First day, ears open:

1. 10 trials—W = 8, P = 0, I = 0.
2. 10 trials—W = 4, P = 1, I = 0.
3. 10 trials—W = 7, P = 0, I = 0.
4. 10 trials—W = 5, P = 1, I = 0.
5. 10 trials—W = 6, P = 1, I = 0.
6. 10 trials—W = 3, P = 0, I = 0.

First day, ears closed:

1. 10 trials—W = 0, P = 0, I = 4.
2. 10 trials—W = 1, P = 6, I = 3.
3. 10 trials—W = 0, P = 3, I = 7.
4. 10 trials—W = 0, P = 0, I = 10.
5. 10 trials—W = 3, P = 6, I = 1.
6. 10 trials—W = 3, P = 3, I = 1.

Last day, ears open:

1. 20 trials—W = 4, P = 2, I = 0.
2. 20 trials—W = 5, P = 2, I = 0.
3. 20 trials—W = 1, P = 5, I = 1.

Last day, ears closed:

1. 20 trials—W = 0, P = 1, I = 18.
2. 20 trials—W = 1, P = 2, I = 15.
3. 20 trials—W = 0, P = 6, I = 12.

It is evident that the sound of the hammer striking against

the glass has been an important element in the preceding reactions.

At this point in the experiments the head rest was removed and during the rest of the investigation the subject sat free but with his face close to the glass as before.

The conditions of the series just described were now altered by the introduction of another element. The subject made his muscles tense leaning slightly forward in his chair and grasping his knees firmly with his hands. The result, however, did not differ greatly from the preceding. The reactions for the last day were—

Ears open:

1. 20 trials—W = 0, P = 8, I = 9.
2. 20 trials—W = 0, P = 8, I = 11.
3. 20 trials—W = 1, P = 12, I = 1.

Ears closed:

1. 20 trials—W = 3, P = 0, I = 15.
2. 20 trials—W = 0, P = 1, I = 18.

The subject had a feeling during this series that the muscles of the arms and legs were too far removed from the eyes to make their contraction effective in inhibiting the wink and so this gave way to contraction of the facial muscles and those of the neck but the reactions continued about the same.

The contraction was then extended to the muscles of the head and scalp. The effort was so great as to throw the face into a quiver of distortion. At the same time a control series was introduced in which effort was directed merely to inhibiting, but without any unusual muscular tension. The following are the reactions for the last of five days' practice.

Ears open:

1. 20 trials—W = 0, P = 3, I = 17.
2. 20 trials—W = 0, P = 1, I = 19.
3. 20 trials—W = 0, P = 2, I = 18.

Ears closed:

1. 20 trials—W = 0, P = 2, I = 17.
2. 20 trials—W = 0, P = 3, I = 17.

Three "control" series taken on the last three days gave

1. 20 trials—W = 0, P = 18, I = 0.
2. 20 trials—W = 0, P = 8, I = 10.
3. 20 trials—W = 3, P = 1, I = 0.

It is evident from these results that contraction of the muscles of the scalp together with the other muscles of the head greatly increased the power to inhibit. This means of control was not only the most effective but it also retained its influence longer than the others. In time, however, it, too, lost its hold but as its effect lessened, the general power to control

without the aid of any muscular contraction seemed to increase. As this improvement had not been observed while the practice was confined to attempts to inhibit directly, it seemed worth while to try to determine how much time would be needed for learning to control this reflex through adjacent muscles, and the experiments directed toward this will be discussed later.

A series in which contraction of the face and scalp muscles was alternated with some in which the hands were tightly clinched clearly showed the greater effectiveness of the former in facilitating control, and finally the effect of slightly bending the head forward without any intentional muscular contraction was tried. This gave a feeling of security on account of the over lapping eye brows and the position necessarily occasioned some contraction which doubtless aided in the control. The result was much the same as when the face and scalp muscles were contracted.

In order to test the effect of distraction of the attention without muscular contraction, a series was taken in which the subject added columns of figures on a card behind the hammer. These were alternated with others in which there was only effort to inhibit. In order that the effect of previous practice in inhibition through muscular contraction might be eliminated a new subject was taken for this series. The results failed to show any effect of this kind of distraction on the wink reaction. The experiments were continued for twenty days.

Having found that we may learn to control the reflex wink by beginning with muscle situated near the eyes and gradually getting hold, as it were, of the eye muscles themselves, the question suggested itself as to whether this control could be gotten in a short time or whether the long and tedious training that our subject went through is necessary. Eight subjects in all were tested. Of these two succeeded after the first or second excitation and repeated trials failed to bring any noticeable reaction afterward.[1] Two were unable to learn to inhibit within a reasonable time, *i. e.*, two or three weeks, and the progress of the others is shown below.

The instructions were necessarily rather general. They were to contract the facial muscles and then to try by elimination to get control of just such muscles as proved effective in the inhibition.

Subject A: First series, Dec. 5.
20 trials—W = 4, P = 5, I = 3.
Subject A: Final series, Dec. 13.
20 trials—W = 0, P = 4, I = 16.
Subject B: First series, Dec. 4.
20 trials—W = 1, P = 15. I = 2.

[1] *Cf.* also the experiments of Partridge (17.)

Subject B: Final series, Dec. 9.
20 trials—W = 0, P = 1, I = 18.
Subject C: First series, Jan. 7.
20 trials—W = 1, P = 12, I = 7.
Subject C: Final series, Jan. 11.
20 trials—W = 0, P = 0, I = 20.
Subject D: First series, Dec. 30.
20 trials—W = 6, P = 5, I = 0.
Subject D: Final series, Jan. 3.
20 trials—W = 1, P = 5, I = 14.

The evidence of those who succeeded was that they began with mass muscular control and gradually worked down to the essential ones by "getting their feeling." The time required was about a week. Two succeded in five days. At the last test there was no muscular contraction noticeable to the experimenter and the subjects themselves felt no more eye tension, as they said, than would be necessary to observe a fine point at a little distance.

That this control is not due to distraction of the attention was shown by a further experiment with D immediately after his final test. He read aloud rapidly while the hammer struck against the glass as before, holding his book in such a position as to bring the hammer within the margin of the field of vision. The result was

20 trials—W = 3, P = 8, I = 0.

We may then summarize the results of this part of the investigation as follows:

1. The eye reflex is a complex reaction resulting from a combination of visual and auditory sensations and the final effect seems to be greater than the sum of the separate effects. Probably the order in which the stimuli are experienced has something to do with this.
2. Moderate fatigue does not noticeably affect the reaction.
3. Attention on inhibition only slightly reduces the number and intensity of the reactions.
4. Attention on distracting objects gives moderate and temporary results.
5. Distraction of the attention by various devices lessens the reaction but they soon wear out.
6. Adding has no appreciable effect.
7. Contraction of muscles remote from the eyes does not greatly reduce the reactions.
8. Contraction of muscles near the eyes has decided effect.
9. Closing the ears markedly lessens the reaction.
10. The training in control gotten through contraction of the muscles near the eye seems to have an effect on the simple

inhibitory control (*i. e.*, that in which the attention is on inhibiting), increasing its efficiency up to a certain point beyond which a great deal of practice does not carry it.

While the experiments that we have just described were in progress one of the subjects put his twenty-five weeks old baby-girl before the apparatus, the effect of which he had been trying for weeks to learn to resist but with only partial success, and to the surprise of the observers her eyes were wholly unaffected though her face was close to the plate glass, against which the hammer was striking with so much noise that most adults could learn to restrain the wink only after considerable practice, and she was looking directly across the hammer's course. This occurred at the end of the university year, and fourteen weeks later, when the close of the vacation permitted another test, the child winked every time the hammer struck. After two or three blows she looked frightened and turned to her mother, as though about to cry. During the previous test she had been undisturbed mentally as well as reflexively.

These observations suggested enlarging the investigation to include the genesis of the reflex wink, and through the kindness of two sets of parents in the university circle the writer was able to make careful tests of the matter upon two young babies.

For the first series of experiments a baby-girl was the subject. The tests were made during the middle of the afternoon. The conditions of sleep and health were noted when necessary. Tests were begun on her 68th day. On that day I made forty rapid passes with my index finger toward her eyes, almost touching them, but she did not wink. Neither did she when her eye lashes were touched with a handkerchief but if the conjunctiva was touched she usually winked, though not always, and the winks were chiefly confined to a "fresh eye," *i. e.*, she soon became accustomed to it and ceased to respond. This could hardly have been a matter of fatigue of the sense organ or nerves since care was taken to avoid this and the failure to respond was noticed after two or three winks.

Before making the tests with the apparatus we passed a brass spring about the size of the hammer back and forth in front of her, at a distance of two feet from her face, and found that she followed it with her eyes in whatever direction we held it.

Care was taken throughout the investigation to guard against fatigue and, with a baby, it is not difficult to determine when this condition is being approached.

Two sets of experiments, each of twenty trials, were then made with the hammer. In these the auditory and visual excitations were combined, as in the case of the adults. The baby was held so that her face was close to the glass with her

eyes on a level with the hammer as it struck against the other side. The force of the blow was sufficient to give one quite an unpleasant shock.

In the first set of twenty tests she winked five times and in the second three. But they were not merely winks. The baby started or jumped slightly and the wink was part of this general reaction. This will be found to play an important rôle in the development of this reflex.

On her 75th day the experiment was varied so that we might observe the effect of sudden visual excitation. A piece of soft cloth was folded several times and held between the handle of the hammer and the body of the apparatus so as largely to eliminate the sound of the hammer's blow. This rather primitive method was adopted because after trying several more elaborate plans it was found the best. The problem was to have the hammer fly up with its full force directly against the glass but to strike it so lightly as not to have the sound a disturbing element. To do this it was necessary to stop it suddenly, just before it struck the glass, and the cloth padding did this fairly well. There was usually a little noise but apparently not enough to be a disturbance.

As before, the baby's face was brought close to the plate glass with her eyes on a level with the hammer as it flew toward her. Whenever her eyes wandered from the direction of the hammer the experimenter waited until he could get her attention again or, sometimes, her father walked about with her so as to avoid fatigue and keep conditions as natural as possible.

In the first twenty tests there were two winks but they seemed entirely natural and not caused by the hammer. In order that there might be no mistake about this, one hundred and fifteen additional tests were made in sets of about twenty each, and in no instance was there the slightest trace of a wink except such as occurred between the intervals of the stimulation, and care was taken that the hammer strokes should not follow one another so rapidly as to cause any confusion here.

The experiment of touching her eye lashes with the corner of a handkerchief was repeated and she winked in about seventy-five per cent. of the cases.

On her 82nd day we began three sorts of tests all of which were made each time during the remainder of the investigation. The auditory and visual combined and the visual alone have just been described. In addition we tried the effect of sound alone. In this the baby was held so that her eyes were turned away from the apparatus while one ear was toward the glass and near it.

For convenience I will designate these three sorts of tests as "auditory—visual," "visual," and "auditory."

The visual tests were always made first so as to avoid any possible nervous effect of the noise from the hammer.

82nd day. I. Visual. (Sound largely eliminated by padding.)

1. 25 tests . . . . . 3 winks.
2. 25 " . . . . . 0 "
3. 25 " . . . . . 6 "

While some of these winks seemed natural, this was not true of them all, and it was evident that she was gaining in this reflex.

II. Auditory.

1. 25 tests . . . . . 6 winks.

Several of these seemed natural, but with one or two of them the start, which at first characterized every response, was noticeable. Her general reaction, however, had greatly decreased.

III. Auditory—visual.

1. 20 tests . . . . . 11 winks.

She looked frightened and at times seemed on the point of crying, quite in contrast to her behavior two weeks before, on her 68th day, when her response was more organic than selective.

89th day. She had slept very little during the day and was nervous and fretful both before and during the tests. This explains the irregularity of the results. They are interesting in showing her reaction during sickness but should not be regarded as showing her natural reflex response at this time.

I. Visual.

1. 20 trials . . . 3 winks, but only one of these seemed to be caused by the excitation. The other two had every appearance of being natural.
2. 20 trials . . . 6 winks.
3. 20 trials . . . 0 winks. During this series she was looking directly across the path of the hammer at a little girl in a red dress who was standing at the lower end of the table. Her attention being taken up in this way probably prevented her from winking, notwithstanding the other favorable conditions.

II. Auditory.

1. 20 trials . . . 17 winks.
2. 20 trials . . . 8 winks.

In the second series the first five winks began with the second excitation and followed one another in succession. After that she skipped one, then winked, while the other came toward the last.

III. Auditory—visual.

1. 20 trials . . . 15 winks.

Toward the last she seemed bored but not frightened.

96th day. I. Visual.

1. 20 trials . . . 5 winks and two of these were surely natural.
2. 20 trials . . . 5 winks and one was apparently natural.
3. 20 trials . . . 2 winks but both were evidently natural.
4. 20 trials . . . 13 winks. Only one of these seemed natural but she was looking straight at the hammer and made wink after wink, consecutively, while looking at it. The experimenter took care not to have the stimuli follow one another so fast as to have a summation of effect.

II. Auditory.

1. 20 trials . . . 6 winks. Four of these appeared to be natural.

III. Auditory—visual.

1. 20 trials . . . 7 winks.
2. 20 trials . . . 6 winks but only two seemed to be caused by the apparatus.

103d day. I. Visual.

1. 20 trials . . . 2 winks.
2. 20 trials . . . 4 winks.
3. 20 trials . . . 1 wink.
4. 20 trials . . . 1 wink.

So far as we could judge all of these were responses to the excitation.

In order to see the effect of a larger object we then held her behind a glass door, with her face toward the glass, and struck the opposite side with a black cap.

1. 20 trials . . . 1 wink.
2. 20 trials . . . 1 wink.

II. Auditory.

1. 20 trials . . . 8 winks.

Here a variation was introduced. A piece of card board was tacked to the apparatus in front of the hammer so that she could be held facing it, as in the visual series, but could not see the hammer.

1. 20 trials . . . 17 winks.
2. 20 trials . . . 13 winks.

We concluded, however, that the increased number of winks was an effect of the louder and sharper noise that the card caused the hammer to make rather than the result of facing the apparatus.

III. Auditory—visual.

1. 20 trials . . . 14 winks.
2. 20 trials . . . 11 winks.

Most of the winks in the second series were toward the beginning.

Whenever her attention was taken by discomfort caused by her position she did not wink

110th day.

The tests to-day were not altogether satisfactory because the baby was so restless that it was hard to keep her attention. She was not irritable but was more than usually taken up with herself and the objects around her. She seemed more interested in the experimenter than in the experiments.

I. Visual.

1. 20 trials . . . 10 winks.

Two of these came late and one was undoubtedly natural.

2. 20 trials . . . 9 winks.
3. 20 trials . . . 9 winks.

II. Auditory:

1. 20 trials . . . 3 winks.

All of these came with the start that has characterized all these reactions at the beginning.

We repeated the variation, introduced on her 103d day, of putting a piece of card board in front of the hammer so that she could face the apparatus.

1. 20 trials . . . 9 winks.

Three of these were only half winks but they seemed to be responses to the noise.

2. 20 trials . . . 6 winks.

Four of these were very slow.

III. Auditory—visual.

1. 20 trials . . . 5 winks.

Sucking her fingers seemed to take her attention during this series.

2. 20 trials . . . 7 winks.

117th day. I. Visual.

1. 20 trials . . . 17 winks.
2. 20 trials . . . 12 winks.
3. 20 trials . . . 11 winks.

During the latter part of the third set it was evident that her attention was taken up by something beyond the hammer, and as a result she did not wink.

4. 20 trials . . . 10 winks.

Here, again, she seemed to be looking at some object.

II. Auditory.

1. 20 trials . . . 11 winks.

In addition to these there were four partial winks.

2. 20 trials . . . 4 winks.

During the first part of this series her attention was taken up with trying to do something, and she did not begin to wink until she found that she could not accomplish it and so released her attention.

3. 20 trials . . . 8 winks.

Besides these there were four partial ones. Her attention was again occupied during a part of the time and then she did not wink.

The start which characterized her reaction to auditory stimuli at the beginning has been occurring less frequently and to-day was not noticeable

With the piece of card board in front of the hammer and the baby again facing it we found

1. 20 trials . . . 18 winks.
2. 20 trials . . . 14 winks.

In the second set there were also three partial winks.

III. Auditory—visual.

1. 20 trials . . . 18 winks.

She also moved her eye lids, as though about to react, the remaining two times but did not finish the wink.

2. 20 trials . . . 16 winks.

In addition to these there were four partial winks.

126th day. I. Visual.

1. 20 trials . . . 11 winks.
2. 20 trials . . . 14 winks.
3. 20 trials . . . 13 winks.

Delayed winks were quite noticeable in these series.

II. Auditory.

1. 20 trials . . . 5 winks.
2. 20 trials . . . 7 winks.

The card board variation, again, gave the following.

1. 20 trials . . . 9 winks.

There were also two or three quivers.

2. 20 trials . . . 8 winks.

Besides these, two quivers were noticed but during this series the card board attached to the apparatus seemed to take her attention.

3. 20 trials . . . 4 winks.

III. Auditory—visual.

1. 20 trials . . . 12 winks.
2. 20 trials . . . 14 winks.

In addition to these there were four partial winks. The other two times not a muscle moved.

3. 20 trials . . . 13 or 14 winks.

The uncertainty here arose because we were unable to determine whether one was natural or not.

In an investigation of this sort there is much that figures cannot show. The general organic reaction must be a matter of observation, and I find in my note for this day, "she is evidently becoming less affected by auditory stimuli while her reaction to visual excitations is continually increasing." In reference to this her parents said, also, that she had lately begun to notice everything in the room.

131st day. I. Visual.

1. 20 trials . . . 16 winks.

In addition there was one partial wink.

2. 20 trials . . . 15 winks.

An organic response, somewhat similar to that noticed at an earlier age in connection with the auditory excitations, was observed at about this time in the visual reactions. She sometimes winked with great force, shutting her eyes together tightly, as though the visual excitation disturbed her. This sort of a response to auditory stimuli had long since entirely disappeared.

II. Auditory.

1. 20 trials . . . 3 winks.
2. 20 trials . . . 7 winks.
3. 20 trials . . . 5 winks.

With the card board in front of the hammer.

1. 20 trials . . . 3 winks.
2. 20 trials . . . 5 winks.

III. Auditory—visual.

1. 20 trials . . . 8 winks.

Several times in this series, when she was looking directly at the hammer, she winked fiercely.

2. 20 trials . . . 13 winks.
3. 20 trials . . . 12 winks.

In the last two series she winked whenever she was looking right across the hammer's path and, apparently, at nothing in particular.

141st day. I. Visual.

1. 20 trials . . . 10 winks.
2. 20 trials . . . 11 winks.
3. 20 trials . . . 10 winks.

II. Auditory.

1. 20 trials . . . 3 winks.
2. 20 trials . . . 0 winks.
3. 20 trials . . . 2 or 3 winks.
4. 20 trials . . . 0 winks.

III. Auditory—visual.

1. 20 trials . . . 20 winks.
2. 20 trials . . . 20 winks.
3. 20 trials . . . 19 winks.

The organic response reached its culmination in the auditory—visual to-day. In this series she frequently shrank back and looked frightened, closing her eyes and keeping them closed for several seconds as though the stimulus pained her.

152nd day. I. Visual.

1. 20 trials . . . 10 winks.
2. 20 trials . . . 13 winks.

She seemed somewhat more sensitive than usual in this series.

3. 20 trials . . . 6 or 7 winks.
4. 20 trials . . . 5 winks.
5. 20 trials . . . 5 winks.

II. Auditory.

1. 20 trials . . . 7 winks.
2. 20 trials . . . 7 winks.
3. 20 trials . . . 0 winks.
4. 20 trials . . . 0 winks.
5. 20 trials . . . 0 winks.

III. Auditory—visual.

1. 20 trials . . . 19 winks.
2. 20 trials . . . 20 winks.
3. 20 trials . . . 18 winks.
4. 20 trials . . . 19 winks.

The second subject studied was a baby boy. In his case the investigation was begun at a little earlier stage.

As before, a careful test was made to be sure that he could see an object the size of the hammer at a considerably greater distance than that at which the hammer would approach him.

46th day. I. Visual.

1. 20 trials . . . 0 winks.
2. 20 trials . . . 0 winks.
3. 20 trials . . . 0 winks.
4. 20 trials . . . 0 winks.
5. 20 trials . . . 0 winks.

II. Auditory.

1. 20 trials . . . 20 winks.
2. 20 trials . . . 20 winks.
3. 20 trials . . . 20 winks.
4. 20 trials . . . 20 winks.
5. 20 trials . . . 20 winks.

I have called his reactions to the auditory stimulus winks

but that does not characterize them properly. At every stroke of the hammer he shrank back, much as a small boy does when one makes rapid passes at his face with the hand. And then, in addition, he shut his eyes tightly as though the sound were painful. Still he did not jump, at least not perceptibly. The latter mode of reacting seems to be a later stage of the organic response. (*Cf.* also p. 244.)

It was noticed that after twenty excitations, he sometimes seemed to become accustomed to the stimulus and ceased to react.

53d day. I. Visual.

1. 20 trials . . . 0 winks.
2. 20 trials . . . 0 winks.

II. Auditory.

1. 20 trials . . . 15 or 16 winks.
2. 20 trials . . . 17 winks.

The same shrinking in response to the auditory stimuli was observed only it was a little less marked. The so-called winks are not yet that but rather the participation of the eyes in the general organic shrinking reaction.

60th day. I. Visual.

1. 20 trials . . . 0 winks.
2. 20 trials . . . 0 winks.

These series were unusually successful in the fact that he looked directly through the glass across the field of the hammer all the time. For this reason delays, efforts to get his attention, were unnecessary.

II. Auditory.

1. 20 trials . . . 6 winks.
2. 20 trials . . . 8 winks.

The shrinking, hitherto so noticeable, was hardly apparent to-day. He paid little attention to the sound except for the winking reaction which now for the first time took the place of the shrinking. This was also the first day when he did not act as if the stimulus pained him or as though he wanted to cry.

67th day. I. Visual.

1. 20 trials . . . 0 winks.
2. 20 trials . . . 0 winks.

II. Auditory.

1. 20 trials . . . 9 winks.
2. 20 trials . . . 8 winks.

One or two of the winks in the last series may have been natural.

The shrinking reaction was entirely absent and there was no evidence whatever that the sound disturbed him; indeed, to-

day seemed to mark a turning point in his psychic life, since, for the first time, he turned his head to see what was making all the noise.

74th day. I. Visual.

1. 20 trials . . . 0 winks.

One occurred but it seemed perfectly natural.

2. 20 trials . . . 0 winks.

II. Auditory.

1. 20 trials . . . 15 winks.
2. 20 trials . . . 3 winks.

In the last series his brow contracted seventeen times but in only in the three cases did it go on to the wink. There were thus seventeen reactions to the excitation but they were of varying degree. This may indicate a transitional stage.

3. 20 trials . . . 19 winks.

83d day. I. Visual.

1. 20 trials . . . 0 winks.
2. 20 trials . . . 2 winks.
3. 20 trials . . . 2 winks.

II. Auditory.

1. 20 trials . . . 17 winks.
2. 20 trials . . . 15 winks.

87th day. I. Visual.

1. 20 trials . . . 5 winks.

Three of these were certainly caused by the apparatus, the other two were uncertain.

2. 20 trials . . . 4 winks.

One was accompanied by the characteristic reflex "start" which occurred now for the first time in the visual reactions. (See p. 241, *cf.* also pp. 244 and 245.)

3. 20 trials . . . 2 winks.
4. 20 trials . . . 5 winks.

One or two in the last series seemed natural.

5. 20 trials . . . 5 winks.

II. Auditory.

1. 20 trials . . . 8 winks.
2. 20 trials . . . 6 winks.

On his 71st day his father found that he winked occasionally when he thrust his finger at his eyes but to-day he reacts every time to this stimulus.

It is interesting, in connection with the development of the reflex wink, to learn that on his 86th day, just when the visual reflex was making its most rapid development, the child suddenly burst into a violent fit of crying and hid his face in his mother's lap at the sight of a neighbor's baby.

Unfortunately the experiments were brought to an end at

this time by the baby being taken out of town and it was not possible to test him further until his 130th day.

130th day. I. Visual.

1. 20 trials . . . 5 winks.

In addition to these there were two or three partial reactions, *i. e.*, slight contraction of the brow with noticeable movement of the eyes.

2. 20 trials . . . 12 winks.

II. Auditory.

1. 20 trials . . . 15 winks.
2. 20 trials . . . 13 winks.

I am indebted to Superintendent Harold Barnes for the following note regarding another of his boys. At the end of the baby's eighth week his father noticed that he did not wink however suddenly an object might be brought close to his eyes.[1]

Miss Shinn (19) tells us, too, that her niece winked reflexively for the first time on her fifty-sixth day, when a head was suddenly thrust close to her face.

The following general conclusions may be drawn from this investigation.

1. Until about the fiftieth day babies are excessively sensitive to auditory stimuli.

2. The first response to a sudden sharp sound is organic. It is a general bodily contraction, which may be accompanied by a jump, but if not, it always develops soon into this mode of response, and in this general reaction the eyes participate. The experiments on the baby-girl, the first subject, were evidently begun at about the close of this period. Later, as this investigation shows (and in the case of the little boy, the second subject, it was about the sixtieth day), the bodily response ceases and the wink becomes differentiated as a distinct reaction.

3. The visual reflex does not appear until much later. In the two cases investigated it was first observed shortly after the eightieth day.

4. Sensitiveness to auditory and visual stimuli pursue an inverse development. As the auditory reaction becomes less marked the visual appears and gradually increases in frequency until the adult condition is approached, in which the visual response is common and the auditory rare.

5. Reflex reaction, so far as the wink may be regarded as

[1] Preyer (16) found that his child winked at the quick approach of an object to his face on his sixtieth day. He also noticed the slower closing of the lids than in adults but speaks of it only up to the twelfth day whereas in this investigation it was observed at times in the baby-girl up to her one hundred and twenty-sixth day.

typical, is evidently inherited in the race but learned by the individual. The elements from which the reactions can be built up are given in heredity, but the nicety of their adaptation to ends must be learned.

The manner in which the first subject reacted to the auditory—visual stimulus on her 82nd day and the greater number of winks usually caused by this double excitation is interesting. Can it be that at this early day sight of an approaching object gives new significance to the noise that immediately follows? Her attitude toward it on that day indicated a fear wholly absent in the auditory reactions that just preceded.

The auditory and visual tests were also made on another baby-boy on his 265th day. As the earlier experiments had indicated that withdrawal of the attention from the excitation greatly alters the reaction, two sets of visual tests were taken in alternate series, one such as we have already described while the other differed in having a lighted match held behind the hammer so that, as the baby looked across its path, his attention would be taken by the light. The results are given in the order in which the experiments were made.

265th day. I. Visual.
20 trials . . . 14 winks.
Visual with lighted match.
11 trials . . . 3 winks.
The first seemed to be a natural wink.
Visual.
19 trials . . . 19 winks.
Visual with lighted match.
20 trials . . . 6 winks.
Two of these seemed caused by the sudden appearance of the light.
Visual.
20 trials . . . 10 winks.
Visual with lighted match.
18 trials . . . 3 winks.
One was so gentle that it seemed natural.
In the series with the lighted match the winks came late in the series, usually at the last.

II. Auditory.
20 trials . . . 0 winks.

These experiments were repeated on his 296th day but the match did not hold his attention as before. Whenever his eyes rested on it, however, or on the experimenter, who stood behind the hammer, the reactions were less frequent than at other times.

In order to find out the part played by sound and sight in

the reflex wink of older persons, experiments were made on four university students and on a five year old boy.

In order that the sound might be reduced as much as possible the ears of the subjects were packed with cotton which was kept in place by a bandage passing under the chin. This was

TABLE.

	A				B				C				D				E			
	C	V	A	X	C	V	A	X	C	V	A	X	C	V	A	V¹	X	C	V	A	X
W	6	0	0	0	7	0	0	2	2	0	0	0	20	0	17	0	0	17	0	4	4
W¹	4	1	0	0	7	0	0	0	4	0	0	0	0	1	3	0	0	2	1	1	2
W²	5	4	0	1	2	1	0	0	10	0	1	6	0	0	0	0	3	1	0	1	3
W³	5	5	6	9	4	0	2	2	4	0	2	4	0	8	0	1	2	0	1	7	2
W⁴	0	7	4	5	0	0	8	0	0	0	6	1	0	0	0	0	0	0	0	4	0
O	0	3	10	5	0	19	10	16	0	20	11	9	0	11	0	19	15	0	18	3	9

C = control, *i. e.*, auditory and visual with no attempt to isolate any element in either.
V = visual.
A = auditory.
X = auditory with eyes turned toward the apparatus but hammer covered.
W = full wink, W¹, W², W³, W⁴ = lesser degrees of winking, W⁴ indicating the slightest noticeable twitch of the eyes.
O = no wink.
All of the subjects were university students except "E" who was a boy of five and a half years.
V¹ in "D" gives the results of a further attempt to eliminate sound by having an assistant press the bandages close to the ears of the subject.

in addition to the cloth bumper that broke the force of the hammer as it was about to strike the glass.

The sound could not be entirely eliminated, but this arrangement so muffled it as largely, if not wholly, to neutralize its influence on the reactions.

It is evident from the table that the reaction caused by excitation of the wink reflex, when visual and auditory elements are combined, is the result of both factors, and the part played by each varies with different individuals. In "A" the auditory stimulation was less effective, with "B" they did not differ greatly while in the others the reactions to the auditory was more marked.

Partridge (17), with a similar apparatus, in a series of experiments on children from five to fifteen years of age, found a gradual improvement in control with increasing age, and these results show that Dr. Sanford[1] was right in ascribing much of the effect to the sound of the hammer and to the rattle of the apparatus. Further, the tests on the five and a half year old boy, 'E,' do not indicate any essential difference in the reactions after that age.

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CORRESPONDENCE.

MADISON, WIS., April 2d, 1903.

To the Editors of the *American Journal of Psychology:*

Gentlemen:—

I am much interested in Professor Titchener's plea for summaries and indexes in connection with psychological papers. I entirely agree with him as to the very special value of the summary, no matter whether the paper be short or long. I cannot at all agree with him, however, as to the necessity for long papers in presenting experimental results. Professor Sanford is certainly moderate in his estimate that in most cases, where one hundred pages are written, the same statement could have been condensed and better stated in twenty-five pages. Professor Titchener asks for a special index for all papers exceeding twenty-five pages in length. The essential point is, it appears to me, that papers should not exceed twenty-five pages in length. With a few obvious exceptions, it may be stated with some emphasis that a paper recounting the result of an ordinary research can, with skill in presentation, be confined to this limit. Professor Titchener tells us that long papers are inevitable, and are an evidence of an advance in psychology. Unquestionably the researches thus recorded are signs of advance, but the papers themselves are frequently an evidence of the inability of the investigator to use language or to arrange his thoughts economically. My own solution for the difficulty would be to insist that the essential parts of the paper, together with the interpretation of the points presented, be stated concisely and forcibly; furthermore, that all details by which the evidence is enforced, and the raw material out of which the conclusions have been drawn, shall again be systematically arranged in a series of appendices. The fault with long papers is largely the promiscuous mingling of all sorts of material, which may have been essential in the conducting of the research, but is not essential to the statement of its results. It is much to the credit of American psychology that it has to such a large extent avoided the undesirable habit of long papers, which have come to be characteristic of many schools of psychological writers.

Believe me, very truly yours,

JOSEPH JASTROW.

I am very glad to find my recommendations of summaries and tables of contents and indexes thus heartily endorsed by Professor Sanford and Professor Jastrow. If our three laboratories will henceforth systematically set a good example in these respects, I have no doubt that others will follow it.

I am glad, also, that the question of length of papers has been brought into open discussion. Personally, I have for some time felt that our magazine articles in general are getting to be too long. I tried to put this feeling into practice by cutting down my paper in the Wundt *Festschrift* (with 16 figures in the text) to 25 pp. Apparently the feeling is shared by other American psychologists: for I find that, in the same *Festschrift*, Angell takes only 22 pp., Cattell 6, Judd 17, Pace 15, Scripture 17 and Stratton 25.

At the same time, I am sure that the question is much less simple than Professor Jastrow makes it. Neither the 25 pp. limit nor the plan of appendices will work in every case. Take Angell's paper on sound intensities, in *Phil. Stud.*, vii. It fills 55 pp.; and I fail to see either that it could have been further condensed (it is rather over-condensed already) or that anything at all would have been gained by relegation of parts of it to an appendix. Besides, all men do not write in the same way. Consider the relative length of the sentences in the work of Lipps and of Meinong. Both men have a good deal to say; each says it best in his own way; it would be absurd to run both into the same mould. And I am convinced, also, that one reason for the increasing length of papers is, really, that writers nowadays have more psychological material; the intrinsic quality of the output is, perhaps, no better than it was at first, but we have better methods, and get more facts.

We must remember, too, that perhaps in the majority of cases the longest papers are theses, first attempts at serious writing. The editor's duties in such a matter are very delicate. We have a protectorate, which must in some way be reconciled with independence of the writers. We can correct, advise, suggest, set an example; we can hardly do more without infringing on the author's rights, or at least hurting his feelings.

On the whole, therefore, I think it unwise to set a definite limit to the length of papers or to prescribe a plan for their arrangement. I want every one who has something to say to say it, and to say it as he likes best: only I ask that he give the reader such mechanical aids to the comprehension of his writing as he readily can. Further editorial legislation, such as Professor Jastrow suggests, would cripple many young investigators, who learn the better path by experience. You cannot create style by act of parliament. But, I may repeat, this editorial conservatism does not affect my belief that most of the things we read might, as a matter of fact, have been considerably shortened.

E. B. TITCHENER.

LITERATURE

Agnosticism. By ROBERT FLINT, D. D., LL. D., F. R. S. E., Professor in the University of Edinburgh. New York, Chas. Scribner's Sons, 1903. pp. xviii, 664. Price, $2.00.

"The present volume," Professor Flint tells us, "is part of what was many years ago announced as meant to form when completed a System of Natural Theology which would deal with four great problems." The first problem, that of evidence for belief in the existence of God, was dealt with in his *Theism;* the second, the refutation of antitheistic theories, was partly—as regards atheism, materialism, positivism, secularism, pessimism, and pantheism—taken up in his *Anti-theistic Theories*, and is, as regards agnosticism, taken up in the present volume. The two remaining problems, the delineation of the character of God, as disclosed by nature, mind and history, and the tracing of the rise and development of the idea of God, have been treated only so far as they come under discussion in the article *Theism*, in the 9th. edn. of the *Britannica*.

The present work, then, is not an historical, colorless estimate of the part played by Agnosticism in modern thought; it is a partisan work, destined to refute the agnostic. Let us see how it opens.

Professor Flint begins by giving Hutton's and Huxley's accounts of the coinage of the term. According to Hutton, Huxley suggested the word *before* the formation of the Metaphysical Society, taking it from St. Paul's mention of the altar "to the unknown God." According to Huxley, it was coined *after* the formation of the Metaphysical Society, "as suggestively antagonistic to the 'gnostic' of Church History." Now one of these accounts must be false if the other is true. Professor Flint remarks that both "well deserve to be borne in mind," and appears to think that both alike represent the facts. Possibly they do; possibly the term is of mixed associative origin. Nevertheless, the reader is not reassured by finding that the author, in his haste to enter upon detailed destructive criticism, allows the discrepancy of the two statements entirely to escape his notice.

Professor Flint next revives the etymological argument. "It was contrary to Greek usage to terminate with *ikos* a word which commenced with *alpha privativum*." The fact is, that it was contrary to classical Greek usage to terminate with *ikos* a word so beginning, in which the negative meaning was retained. If the negative meaning dropped out, and the word became positive, the formation was allowable: witness the adjective *aoristikos*. In so far, then, as agnosticism represents a positive and not merely a negative tendency (as it surely must, if it takes 664 pages for its refutation), the most classical of ancient Greeks would have had no objection to the word. But, Greek philology apart, what does the argument matter? If the genius of the English language allows and has for a long time past allowed such formations, why need the modern Englishman trouble about the ancient Greek? If Professor Flint will consult the *A* in any historical English dictionary, he may perhaps be induced to withdraw this part of his indictment.

We are next told that Huxley had better have called himself a 'sceptic,'—'sceptic' being a good old philosophical word that meant

the same thing as 'agnostic.' True, "it must be admitted that it has acquired an offensive connotation." But is a man bound to call himself by a name that has acquired an offensive connotation? Granted that 'agnostic' has acquired just the same connotation—a statement, however, that does not accord with the later statement that "the man who calls himself an agnostic implicitly claims to be no common man, but a philosopher"—granted this: may not a man coin a word to get a breathing-space for himself and his opinions in the push and hustle of orthodoxy?

"The criticism in which I have thus far indulged may seem to some of my readers rather hypercritical." Let us hope so, if we hope for any fairmindedness in the reading public. But stranger things are to come. We find Professor Flint deciding, with his eyes open, that he cannot meet agnosticism on its own or indeed on any common ground. "It is in vain for a non-agnostic [a pretty term, if we are considering terms!] to seek to find a definition of agnosticism which will satisfy an agnostic. Any definition of agnosticism which will satisfy an agnostic must of necessity fail to satisfy a non-agnostic." We are taught in science that the definition of a standpoint or movement or attitude should come after the standpoint or movement or attitude has been thoroughly canvassed, in its historical bearings; the definition should be inductive and impartial, and may obtain whether we accept or reject the mode of thought defined. Not so, it would appear, in a System of Natural Theology! First of all, you have the right to define a movement at the outset to suit your own ideas of it. Secondly, it is true, you have the duty of not "judging any system merely by the definition" that you give. But—either your definition prejudices you, and so you *do* do things "unfair and unreasonable:" or your definition must be modified by your treatment of the systems, and then it has needlessly prejudiced your agnostic readers. It is hardly necessary to say that the 'duty,' beautiful as it looks in theory, does not affect Professor Flint's practice. He starts out with a definition which no agnostic would accept, and, at the end, finds that definition triumphantly vindicated by his discussion of the systems.

Once more, and we have finished with this analysis. Professor Flint assures us that he has no animus against Huxley. "Great Britain may well be proud to have had such a son as Thomas H. Huxley. I must reject any view of his which seems to me erroneous; but the fact of a view being his can never, I feel sure, be among my motives or reasons for rejecting it." Imagine this sort of preface to a piece of scientific criticism! One who has positive arguments to adduce need not apologize for adducing them: we are all working in the interests of truth, and are grateful enough—though we may be sore for a moment—if our clay idols are broken. But when a man comes to us with the assurance that these idols, after all, were delicately moulded and had their value as works of art, we rather suspect him of the very animus which he is at pains to deny.

We have dealt only with the first 20 pages of the 664. We have, however, probably said enough to give the reader an idea of the contents of the book. It is comprehensive, scholarly, erudite—and *ex parte* from cover to cover. For those that like their history of philosophy written in this way, this is the sort of book that they will like. The rest—we hope the majority—will use the book as a work of reference, as saying the worst that can be said against a great movement of modern thought, but will hardly trust it as a guide to the course of that movement.

P. E. WINTER.

Experimental Psychology and its Bearing upon Culture, by GEORGE MALCOLM STRATTON. The Macmillan Company, N. Y., 1903. pp. vi+331.

The author has undertaken to state the standpoint of the New Psychology, to pass in review some of the chief results already attained by it, and to assess the value of those results for culture and life. This general problem determines the subject matter treated. There is first an historical introduction; a second chapter deals with the general character of the psychological experiment; a third with the possibility of mental measurement; two with unconscious ideas; one with illusions; two with space perception; two with memory; one with imitation and suggestion. The remaining chapters indicate the application of the results of psychology to culture. There are two on æsthetics (the enjoyment of sensations and their forms; the color and differentiation of the Fine Arts), and two on the philosophical and ethical bearings of the new science. These discuss the connection of mind and body and the spiritual implications of experimental work.

The historical introduction begins with Aristotle and ends with Wundt. English empiricism, Berkeley's work in vision, the evidence of surgery in the removal of cataracts, Gœthe's experiments with colored glass, the personal equation of the astronomers, Helmholtz' work in optics and sound, and the phrenologists Gall and Spurzheim are all shown to have had more or less influence upon the development of the New Psychology. The immediate ancestry of the science is, however, traced to Weber, Fechner and Wundt. Of Weber it is said (p. 10) that he not only aroused interest in his results, but also made men recognize the experimental method as a mode of procedure in psychology. This statement attributes to Weber much more than is his just due. Weber was first and last a physiologist; his work on the sensitivity of the skin and on the sensible discrimination of weights is wholly physiological in character. His theory of sensation circles (a purely physiological conception) and the absence of any psychological insight in the *Tastsinn und Gemeingefühl* are sufficient evidence for this statement. Of Fechner it is said, that he tested Weber's Law by many thousands of experiments in lifting weights; on the basis of these results, he cast Weber's Law into a mathematical formula. And, further, having satisfied his mathematical impulses, he fell to developing the philosophical implications of his formulæ (p. 12). This certainly, is an unwarranted misrepresentation of Fechner's service to experimental psychology. His place as one of the founders of the science rests upon psychological work of the first order, quite independent of psychophysics; for example his work on after-images. Even James, who has little love for psychophysics, admits that Fechner had great psychological insight. And, furthermore, Fechner's philosophical system was already developed long before he began his work on psychophysics. The *Elemente*, as a matter of fact, was undertaken to prove his philosophical theory, and not conversely.

The treatment of mental measurement is curious. It begins by showing that some mental processes can be measured. Thus, one is able to measure reaction times. One is also able to determine the amount of contrast induced by a complementary color. But the issue is again raised whether, in the instances cited, any mental process is measured. The question is further considered, in a discussion of four kinds of psychical quantity: these are intensity, temporal and spatial quantity, and simple enumeration. (i) *The quantity of intensity.* The objection so often made to Fechner's conception of intensive magnitudes, that no sensation intensity is made up of smaller intensities, is met by the reply that although an experience is indivisible it

may be none the less quantitative. Although the author seems to think that this reply meets the objection, he is inclined to believe that even if intensity should fail, there is still hope for mental measurement in the other quantities. (ii) *Spatial quantity*. The argument in this case is, that space is not a universal form of mental process; but that the object which we have in mind may be spatial, without the higher mental processes sharing in this property. As an example of this sort of 'non-physical' spatial quantity, is cited the degree of divergence produced in two parallel lines by cross-hatching. (iii) The psychologist has good grounds for assuming *temporal quantity* within his own field. (iv) The quantity of *simple enumeration* depends upon the proposition that wherever any real differences exist, the notion of quantity and number also exists. In this sense, quantity is as much a mental attribute as a physical one. From this discussion, the author concludes that "mental phenomena are quantitative." This treatment is unusual, in that it wholly disregards the development of the concept of mental measurement, in the hands of Fechner, Delbœuf, Müller, Stumpf, Wundt and Ebbinghaus.

The chapters on illusions, space perception and memory ought to do great good, not only in the way of popularizing the results of psychological investigation, but also as proof of the efficiency of the science itself. And the chapters on imitation and suggestion should be of value, as an antidote to popular superstition regarding hypnotism and spiritualism.

There are minor slips. Thus, the statement (p. 9) that Vierordt made a study of the time sense more than half a century ago, is not true; the work was published in 1868. Taken as a whole, however, the book is a serious piece of work. It should do much to remove misconceptions, and to give a proper understanding of the standpoint and results of experimental psychology. H. C. STEVENS.

Sprachgeschichte und Sprachpsychologie, von W. WUNDT. Leipzig, W. Engelmann, 1902. pp. 110. Price Mk. 2.

This work is primarily a reply to B. Delbrück's criticism of the *Völkerpsychologie* in his *Grundfragen der Sprachforschung*, 1901. It contains supplementary essays on gesture language, phonetic change, the fundamental questions of syntax and the origin of language. Especially interesting is the introductory chapter, which differentiates the Herbartian psychology, with its application of psychological norms *to* language, from modern psychology, which derives psychological laws *from* language. Interesting, too, is the proof of survival influences of the older classical philology and of romanticism upon the current science of language. The work as a whole forms a valuable addition to the discussions of the *Völkerpsychologie*.

Le mensonge: étude de psycho-physiologie pathologique et normale, par G. L. DUPRAT. Paris, F. Alcan, 1903. pp. 190. Price fr. 2.50.

M. Duprat is already well known as a writer on mental pathology, as the author of an *Éthics* which has recently been translated into English, and as the translator of Baldwin's *Social and Moral Interpretations*. His present work is based upon returns to a questionary regarding children's lies, but refers also to lying as it is found in uncivilized peoples, and among civilized adults, normal and abnormal. He concludes that lying is dependent upon tendencies which have their roots in character, in affective disposition, in physiological constitution and in neuro-muscular diathesis. His remedy is the instillation of "true ideas and generous sentiments," and the development of a critical sense, by scientific education.

A Philosophical Essay on Probabilities, by PIERRE SIMON, Marquis DE LAPLACE. Translated from the 6th French edition by F. W. TRUSCOTT, Professor of Germanic Languages, and F. L. EMORY, Professor of Mechanics and Applied Mathematics in the W. Virginia University. New York, J. Wiley & Sons, 1902. pp. iv, 196. Price $2.00.

The first thing that strikes one about this book is that it has no index. The second is, that it has no notes, not even to the historical ch. xviii. And the third is that the translators have set themselves a task that is too high for them. They are evidently unfamiliar with the terms ordinarily employed in Probability; witness their use of 'hope' for 'expectation:' while at times the mere French text has proved too much for them; witness the confusion of 'sol' with 'soleil,' p. 143.

Recherches cliniques et thérapeutiques sur l'épilepsie, l'hystérie et l'idiotie, par BOURNEVILLE. Vol. xxii. Paris, F. Alcan, 1902. pp. clx, 236.

Part i. gives the yearly report (1901) of the Bicêtre and the Fondation Vallée, including an interesting memoir on schools for abnormal children in all parts of the world. Part ii.—Instructions médico-pédagogiques—shows the mode of recording and diagnosing cases received at the Bicetre. Part iii., written by Dr. Bourneville in collaboration with MM. Boyer, Crouzon, Philippe, and others, contains clinical and therapeutical reports and suggestions, together with various notes upon pathological anatomy.

Experiments on Animals, by S. PAGET. With an Introduction by Lord Lister. The Science Series, No. 11, pp. xvi, 387.

This is the second edition of a work issued in England in 1900 by the Secretary of the Association for the Advancement of Medicine by Research. It is an altogether admirable summary of the topic with which it deals. "Its earlier pages," we read in the Introduction, "deal with physiology, the main basis of all sound medicine and surgery. The examples given in this department are not numerous; they are, however, sufficiently striking, as indications that, from the discovery of the circulation of the blood onwards, our knowledge of healthy animal function has been mainly derived from experiments on animals. The chief bulk of the work is devoted to the class of investigations which are most frequent at the present day" (bacteriology, action of drugs). The concluding part of the volume discusses the Vivisection Act of 1876.

Response in the Living and Non-Living, by J. C. BOSE. London, New York and Bombay, Longmans, Green and Co., 1902. pp. xix, 199.

In this work the author has brought together and amplified the results of a series of papers, published between 1900 and 1902, the aim of which is to prove that "living response in all its diverse manifestations is found to be only a repetition of responses seen in the inorganic." He finds in animal, plant and metal the same phenomena of negative variation, the same relation between stimulus and response, the same effect of superposition of stimuli, the same fatigue effects, the same effects of stimulants, depressants and poisons! The papers referred to have been published in reputable magazines, and a part of the author's experimental work was done in the laboratory of the Royal Institution. The tone of the book is confident, even dogmatic; the illustrations are numerous and convincing.

And yet—what is the experimental basis of the conclusions? (1) Certain limited aspects of the changes produced in muscular, nervous and plant tissue by certain modes of stimulation, and (2) certain electrolytic effects appearing when moist conductors are brought into contact with metallic surfaces and these are caused to vibrate! The superficial analogy between these two classes of results (and under the former heading the results are not accurately described) is to read us the riddle of life and the mechanism of life! It may very well be that all these things "are determined . . by the working of laws that know no change, acting equally and uniformly throughout the organic and the inorganic worlds;" at any rate, many of us hope that it is so. But knowledge is not advanced by the ignoring of large classes of facts and the application of a method of crude analogy to the rest.

More Letters of Charles Darwin: a Record of his Work in a Series of hitherto unpublished Letters. Edited by F. DARWIN and A. C. SEWARD. New York, D. Appleton & Co. 1903. Vol. I, pp. xxiv, 494; Vol. II, pp. viii, 508.

This is an extraordinarily interesting book. By the help of unpublished letters and other material not available for the *Life and Letters*, the editors have been able, with very few repetitions from the latter book, to give a practically complete account of Darwin's life work. The letters are grouped under the headings Evolution, Geographical Distribution, Man, Geology, Botany, Vivisection and Miscellaneous Subjects. Both volumes are illustrated: portraits are given of Darwin and his wife, of Romanes, F. Müller, Lyell, Forbes, Hooker, Henslow, Huxley, Gray and others. The editors are to be congratulated upon their completion of a work which will have a permanent value in the history of science.

Pure Sociology, by L. F. WARD. New York, The Macmillan Co., 1903. pp. xii, 607.

This bulky work contains the author's revision of lecture courses delivered in 1897-9 at the Universities of Chicago, West Virginia and Stanford. The writer "regards all social phenomena as *pure* which are unaffected by the purposeful efforts of man and of society itself." He consequently prints, as a sub-title: A Treatise on the Origin and Spontaneous Development of Society. The book falls into three parts: Taxis, which discusses the general characteristics of pure sociology, its subject-matter and methods; Genesis, which treats of the biological origin of the subjective faculties, of social mechanics, statics and dynamics, and of the social forces, ontogenetic, phylogenetic and sociogenetic; and Telesis, which deals with the biological origin of the objective faculties, the conquest of nature, and the socialization of achievement.

Les grand philosophes: Aristote. Par C. PIAT. Paris, F. Alcan, 1903. pp. viii, 396.

M. Piat is the editor of the collection entitled *Les grand philosophes*, nine volumes of which have already appeared, among them a study of Socrates from his own pen. The present work is a useful monograph on the Aristotelian system. It is divided into four books: Being (definition of first philosophy; determination of the categories; substance; the derivatives of substance; causes), Nature (movement; the unmoved mover; the heavens), Mind (mind and its faculties; nutrition; sensation; thought; desire), and Conduct (the individual; the family; the city). A Conclusion traces the course of naturalism from Plato to Aristotle, and from Aristotle to Strato. The book is fully

annotated, and contains a working bibliography. The writer is especially interested in the relation of Thomas Aquinas to Aristotle.

Les obsessions et les impulsions, par A. PITRES and E. RÉGIS. Paris, O. Doin, 1902. pp. 434.

This little work follows the regular programme of definition, description and classification, records of cases, diagnosis and treatment, and legal aspects of the disorders treated. Obsession is "un syndrome morbide caractérisé par l' apparition involuntaire et anxieuse dans la conscience de sentiments et de pensées parasites qui tendent à s' imposer au *moi*, évoluent à côté de lui malgré ses efforts pour les repousser et créent ainsi une variété de dissociation psychique dont le dernier terme est le dédoublement conscient de la personnalité." "L' impulsion morbide est, dans le domaine de l' activité voluntaire, la tendance impérieuse et souvent même irresistible au retour vers le pur réflexe." The authors cover a wide range of fact, and write with sanity and reserve.

La mimique, par E. CUYER. Paris, O. Doin, 1902. pp. 366.

A careful study of expressive movements, illustrated by many original drawings. The author analyzes the expressive movements of face, head, trunk, arms and legs, and gives a 'dictionary' of emotions and their expression. Curiously enough, no mention is made in his bibliography of the works of James and Wundt.

History of Philosophy, by WILLIAM TURNER. Ginn and Co., Boston, 1903. pp. 674.

The best part of this book is entitled "The Philosophy of the Christian Era." Patristic thought is hastened over and scholasticism is treated more fully. A brief account of perhaps a score of the great scholastics, which occupies two hundred pages and much of which has evidently been worked up from original sources, makes it a handbook of much practical value. No Catholic has yet given us such a compendious and valuable classbook on this field, and it is sure to make it all the more worthy.

Outlines of Psychology, by JOSIAH ROYCE. The Macmillan Co., New York, 1903. pp. 392.

This handbook is a striking illustration of its author's versatility. Despite his versatility the author has by no means escaped his philosophical bias. This is seen in the treatment of a number of the topics, more so perhaps in his style, and most of all in the topics he has omitted. Further notice will probably follow.

Contemporary Psychology, by GUIDO VILLA. Swan Sonnenschein and Co., London. The Macmillan Co., New York, 1903. pp. 392. Price, $2.75.

Psychologies nowadays are written from many standpoints and all conceivable inclusions and exclusions. The scope of this work can be seen by the titles of its chapters: history and development of psychology, object and scope, body and mind, methods, psychical functions, composition and development of mental life, consciousness, the law of psychology.

L'Imagination, par L. DUGAS. Octave Doin, Paris, 1903. pp. 340.

This fruitful topic is first discussed in the sphere of sense, will, memory and sentiment; in the second part, creative imagination is treated as spontaneous, reflex, practical, scientific and esthetic. It is especially interesting, giving accounts of theories of the imagination and of images.

The Evolution of Man and His Mind, by S. V. CLEVENGER. Evolution Pub. Co., Chicago, 1903. pp. 615.

The topics treated are as follows: the Aryans, Semites, middle ages, evolution, heredity and degeneracy, superstition, evolution of language and writing, hunger and health, development of mind, evolution of brain, sense and feeling, instincts and emotions, mental diseases, character, sociology, etc. Both the thought and the style of this work are loose. Its range is immense and suggests that the author is writing himself out. Probably he would by no means claim that it makes much original contribution, but it is by no means without interest and value.

Variation in Animals and Plants, by H. M. VERNON. K. Paul Trench, Trübner and Co., London, 1903. pp. 415.

This work discusses measurements of variation, dimorphism, discontinuous and correlated variation, its cause, loss, effects of temperature, light, moisture, salt, food, life conditions generally, and in the third part takes up its relation to evolution. The book is perhaps rather too prominently occupied with the writer's own investigations, but they are interesting and not without value.

Imitation or the Mimetic Force in Nature and Human Nature, by RICHARD STEEL. Simpkin, Marshall, Hamilton, Kent and Co., London, 1900. pp. 197.

The chapters treat of imitation in economics, psychology, ethics, religion, politics, law, custom, fashion, language, poetry, habit, heredity, in the inorganic world, molecular activity, and in reasoning. It treats the subject in a large, general way, without indication of much technical knowledge. Some of the chapters were read before the Literary and Philosophical Society of Liverpool.

Control of Heredity, by CASPER REDFIELD. Monarch Book Co., Chicago, 1903. pp. 343.

This is a very unique work and seems to mark a wholesome transition from the old standpoint of phrenology. The writer discusses inheritance, variation, selection, races of men, eminent families and men, reproduction, longevity, etc. His chief effort seems to be on the basis of a large number of individual cases to maintain the thesis that great men are most likely to be born from post-mature parents.

Essai de Classification Naturelle des Caractères, par CHR. RIBÉRY. F. Alcan, Paris, 1902. pp. 199.

Undaunted by the very limited success of previous efforts in this direction, this author presents a new attempt to classify character, discussing first the difficulties of the problem, the relations between intelligence and character, sensibility and will, emotion and passion. He then classifies temperaments as sensitive-active, amorphous, and tempers, with a final chapter on the education of character.

The Human Race. A sketch of classifications. A chapter in anthropology, by DUREN J. H. WARD. Privately printed. pp. 26.

This little pamphlet is interesting and touches briefly upon many topics. It is the smallest, most condensed, and most systematic epitome of its general topic, perhaps, that now exists.

United States Census Office. Manual of International Classification of Causes of Death. Adopted by the United States Census Office for the Compilation of Mortality Statistics, for Use Beginning with the Year 1900. Prepared under the Supervision of W. A. King. Govt. Print, Washington, 1902. pp. 177.

This elaborate classification of the causes of death is prefaced by an

interesting discussion of the methods of securing uniformity and of the errors in reporting and classifying the causes of death. This certainly suggests a new topic for medical education.

Native Institutions of the Ogowe Tribes of West Central Africa, by R. L. GARNER. Reprinted from the Journal of the African Society, 1902.

This reprint is from the proceedings of a society founded to rescue the memory of vanished races and to work against the extinction of those that yet remain. It is a very interesting and sympathetic study of fetichism and magic.

Les Obsessions et la Psychasthénie, par F. RAYMOND et PIERRE JANET. F. Alcan, Paris, 1903. pp. 543.

This is the second number of the fourth series of publications of the fruitful clinic at La Salpêtrière. It is devoted to neurasthenia, aboulia, defective sentiments, general agitations, algias and phobias, delirium of touch, tics, doubt and folly, obsessions, impulsions, and their treatment, with twenty-two cuts. It is composed on the same plan as a former work of these authors in 1898 entitled Nevroses et les Idées Fixes. With the preceding volume, it is believed that data are now supplied for methods of presenting the evolution of the types of these maladies. Janet, in particular, has devoted himself to descriptive psychology in this field, and the two works together make a connected and a more or less complete whole.

Recherches Cliniques et Thérapeutiques sur l'Épilepsie, l'Hystérie et l'Idiotie, par BOURNEVILLE. F. Alcan, Paris, 1902. pp. 236.

The first part of this work is devoted to the history of this service during the year 1901; the second, to medico-pedagogical instruction; and the third, to clinical therapeutics and pathological anatomy.

Le Mensonge, par G. L. DUPRAT. F. Alcan, Paris, 1903. pp. 188.

Lies are first described and classified. Another chapter treats their abnormality. The third is devoted to the lies of childhood. Then follow lies in collective life, in comparative psycho-sociology, their psycho-physiology, lies from the moral and educational point of view respectively.

L'Image Mentale (Évolution et Dissolution), par J. PHILIPPE. F. Alcan, Paris, 1903. pp. 151.

In the first chapter, the mental image is analyzed; the second is devoted to fusion of images; and the third to evolution of the mental image. The work has an experimental basis and is a real contribution to the subject.

Le Dieu de Platon d'après l'Ordre Chronologique des Dialogues, par PIERRE BOVET. H. Kündig, Genève, 1902. pp. 186.

The first part résumés Plato's views on the place of God in his philosophy and in the dialogues which treat of ideas, while the second part is devoted to the God idea of the later dialogues.

Spinoza's Political and Ethical Philosophy, by ROBERT A. DUFF. (James Maclehose and Sons, Glasgow.) The Macmillan Co., New York, 1903. pp. 516. Price, $3.50.

This book is the first part of a task which has occupied the author for many years and which he hopes to complete, but is a whole by itself. It is solely an elucidation and not a criticism. It seeks to furnish a connected and continuous account of Spinoza's system and to show how his ideas were related to each other. It is a work of great value

to the scholar, shedding light upon many of the most difficult questions connected with this remarkable mind.

Aristote, par CLODIUS PIAT. F. Alcan, Paris, 1903. pp. 396.

The editions, versions, commentaries, monographs and special studies are first described. Then the works, beginning with metaphysics, are briefly discussed under the heads—being, nature, soul and human action. It is a monograph of the Aristotelian system which will serve both the expert and the cultivated general reader.

The Seven Cardinal Virtues, by JAMES STALKER. Hodder and Stoughton, London, 1902. pp. 131.

These are wisdom, courage, temperance, justice, faith, hope, and love, each of which has a chapter. It is on this frame work that the moral system of Aquinas is built. The Seven Deadly Sins was the fit title of a former booklet.

Why the Mind Has a Body, by CHARLES A. STRONG. The Macmillan Co., New York, 1903. pp. 355.

This is a work of love and has absorbed the thought of an earnest, able man for at least a decade. It is divided into three parts—the empirical, which considers the facts and the question of causal relations; metaphysical, which discusses metaphysical principles and their application to problem, with a criticism of theories. We hope to print a fuller review later.

Le Goût, par L. MARCHAND. O. Doin, Paris, 1903. pp. 330.

This work attempts to be a comprehensive account of the sense of taste. It includes a series of original, interesting experiments, and contains thirty-three cuts illustrating the nerves involved, their mechanism, etc.

Morale Essai sur les Principes Théoriques et leur Application aux Circonstances Particulières de la Vie, par HARALD HÖFFDING. Schleicher Frères et Cie, Paris, 1903. pp. 578.

This is a very important work. It discriminates positive and scientific morals from theological and philosophical. The author then proceeds to discuss the methods of ethics, the theory of conscience, liberty and will, the problem of evil and good, of individual and social morality. Under the former, he discusses the affirmation of self, suicide, independence, development and love of truth. Under social morality, he discusses the family, various forms of marriage, culture, its materials as ideals and types, the relations between the church and the State, and philanthropy.

David Hume and His Influence on Philosophy and Theology, by JAMES ORR. Charles Scribner's Sons, New York, 1903. pp. 246. Price, $1.25.

A clean, attractive book describing Hume's life, his literary labors, relations to previous philosophy, and the chief problems which he discussed, viz.: the first principle of knowledge, cause and effect, free will, substance, the material world, the ego, utilitarianism, miracles, political economy and miscellanies, with an account of the different editions of his work.

De l' Étude des Phénomènes au Point de Vue de leur Problème Particulier, par GASTON GAILLARD. Schleicher Frères et Cie, Paris, 1903. pp. 245.

The issues of philosophical research into particularities, development of algorithms, anti-moralism, special problems and their groups, are treated in this work.

An Introduction to Systematic Philosophy, by WALTER T. MARVIN. The Macmillan Co., New York, 1903. pp. 572.

This is the most comprehensive book known to the present reviewer. It discusses first the philosophy of nature, then that of mind; ontology, cosmology and cosmogony follow. Part two treats the nature of knowledge, its validity; the world as presupposed by knowledge and its manifold interpretation. Part three treats of the philosophy of religion, theoretical ethics, æsthetics and philosophy as a science.

The Influence of Emerson, by EDWIN D. MEAD. American Unitarian Association, Boston, 1903. pp. 304.

This work, dedicated to Edward Everett Hale, has three parts: (1) the philosophy of Emerson, (2) Emerson and Theodore Parker, (3) Emerson and Carlyle. No better man could be chosen to write such a book than Mr. Mead, and it goes without saying that his work is not only timely but one which all lovers of Emerson will wish to read.

Archiv für die Gesamte Psychologie, von PROF. A. KIRSCHMANN in Toronto (Canada), PROF. E. KRAEPELIN in Heidelberg, PROF. O. KUELPE in Würzburg, DR. A. LEHMANN in Kopenhagen, PROF. G. MARTIUS in Kiel, PROF. G. STÖRRING in Zürich, DR. W. WIRTH in Leipzig and PROF. W. WUNDT in Leipzig. Wilhelm Engelmann, Leipzig, 1903. Vol. I, Parts 1–3.

This new Archiv contains the following foreign papers: measurement of fatigue by Kraepelin; the influence of accessory stimulation on perception of space by Pearce; the possibility of a quantity of tone sensation by Gätschenberger; a report of progress in language of children during the four years ending in 1902 by Gutzmann, inner nutrition and organic sensations by Lipps; difference of tone and consonance by Krüger; on the single and collective achievments of school children by Mayer.

Von der Nervenzelle und der Zelle im Allgemeinen, von PAUL KRONTHAL. Gustav Fischer, Jena, 1902. pp. 274.

This work is divided into two parts, one on the biology of the nerve cell, and the other on cell in general and the nerve cell in particular. At the end are eleven full page reproductions of microscopic specimens in color.

The University of Colorado Studies, edited by Arthur Allin and Francis Ramaley. University of Colo., 1903. Boulder, Vol. I, No. 3. pp. 262.

Dr. Allin has two interesting papers, one on the basis of sociality and the other on the law of future specific and social efficiency.

Studien zu Methodenlehre und Erkenntnisskritik, von FREDERICK DREYER. Wilhelm Engelmann, Leipzig, 1903. pp. 498.

This volume discusses the method of continuity, meta-geometry, possibility, natural and associative complexion, actuality, etc.

Das Ethische Element in der Asthetik Fichte's und Schelling's, von BENJAMIN F. BATTIN. Kampfe, Jena, 1901.

This thesis discusses chiefly Kant, Fichte and Schelling.

Neurasthenie und Hysterie bei Kindern, von DR. ALFRED SAENGER. S. Karger, Berlin, 1902. pp. 32.

Anthropological Instruction in Iowa, by DUREN J. H. WARD. Reprinted from the July, 1903, number of the Iowa Journal of History and politics, published at Iowa City by the State Historical Society of Iowa. pp. 29.